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(Continued from February number)

Morning Session, Tuesday, December 29, 1908

The meeting was called to order by First Vice-President Britton at 10.10 a. m.

CHAIRMAN BRITTON: The meeting will please come to order. The first number on the program will be two papers by Mr. J. G. Sanders.

(Abstracts of these papers were presented.[1])

NOTES ON INSECT PHOTOGRAPHY AND PHOTOMICROGRAPHY

By J. G. SANDERS, *Washington, D. C.*

In response to the requests of several entomologists interested in photography of insects, a few notes on this subject are herewith offered. Limited space precludes a full discussion of this interesting work, but a few helpful suggestions may minimize the bugaboo of intricacy which deters many from attempting the work and hinders others in attaining good results.

The desirability of good photographs of insects is becoming more apparent as our critical study of those creatures advances. In this

[1]The first paper, entitled "The Identity and Synonymy of a Few of Our Common Soft Scales (Coccidæ), will be published in the next issue of the JOURNAL.

neglected phase of entomological work great things are in store for the careful operator, and surprising results can be obtained with good lenses properly selected for the work at hand. A certain life-like and characteristic reproduction of general appearance and of minute surface detail that can not be duplicated by pen or brush, is obtainable by means of the photographic plate. On the other hand, portions of insect anatomy can be portrayed more satisfactorily by a line drawing which defines and accentuates outlines.

Since the ordinary photograph is composed entirely of light and shade and intermediate tones, it is necessary to study the subject with a view to the best illumination to produce the contrasts and high lights necessary for good detail. Photography of insects and of landscapes are radically different; in the former greatest detail, in the latter a certain indistinct haziness is considered the optimum result.

It is evident that photographs of insects, to be of benefit for illustration and study, must range from natural size to several or many diameter enlargements. By general consent, a photograph of an object from natural size up to ten diameters is called a *photomacrograph;* beyond ten diameters, a *photomicrograph.* There may be some value in this distinction, but the general idea of a photomicrograph is an enlargement of one hundred or more diameters. Photomicrography will be discussed under a separate heading.

Insect Photography

A perfect, well mounted, clean specimen is absolutely essential for a good photograph, since every defect is extremely noticeable at an enlargement of several diameters. Flat and convex insects can be photographed satisfactorily, but must receive different treament to insure that all parts may be in sharp focus.

There will be little excuse for poorly focused pictures of convex objects when it is understood that by "stopping down" the lens (reducing the diaphragm orifice) proportionately greater depth of focus can be obtained. It must be remembered that stopping down the lens reduces the amount of light reaching the sensitive plate, thus requiring much longer exposure.

Apparatus.—It is presumed that the operator has the ordinary dark-room facilities available, and has a general idea of the principles of photography.

An ordinary long bellows camera may be fitted up for temporary use by a handy person. It may be arranged horizontally or vertically, preferably the latter, because of the ease in arrangement of specimens, either dry or immersed in liquid, and the greater facilities of

illumination and arrangement of background. For a large amount of work it is preferable to obtain a photomicrographic stand. The Bausch & Lomb Optical Company, of Rochester, N. Y., has produced one of the best simple stands available for this work. This stand is adjustable for inclination to any angle from horizontal to vertical, and is provided with a 24 inch bellows, with adjustable lens board and ground glass, and carries 4 x 5 inch plates.

There are numerous lenses on the market which may be available for enlarging work. The Bausch & Lomb firm has produced in their Micro-Tessar series the best lenses for this work that are known to the writer. The 72 mm. lens (nearly 3 inch focus), which is extremely rapid, working at f-3.5, is the most serviceable one for ordinary work. With the above photomicrographic stand and 72 mm. lens, photographs ranging from one-half natural size to 8 diameter enlargements can be secured. The 48 mm. and 32 mm. lenses are of higher power. With the latter lens fitted in the above stand 20 diameter enlargements can be made.

The ordinary dissecting stand, with rack and pinion, will be found very useful for focusing. A piece of plain glass to which has been glued a cork fitting the lens holder of the dissecting microscope, can be used for carrying the specimens. When the bellows has been adjusted at the proper length to secure the desired magnification, the focusing can be done with the above arrangement without further change of the bellows.

Preparation of subject.—As before stated, the specimens may be dry or immersed in a watch glass or shallow glass dish, the latter method being applicable to larvæ or specimens in preservative fluids. The fluid has a buoyant effect on the hairs or spines of various hairy larvæ, causing them to assume a more natural appearance while immersed. To prevent shadows, loose specimens should be laid upon a very clear, clean glass as above mentioned, with the background some distance below. For pinned specimens a tiny piece of cork should be glued to the glass plate, thus preventing any shadow from specimen or pin. An old photographic plate, cleared of the film with sulphuric acid, will furnish a good grade of glass for this purpose.

Whenever possible the pins should be removed from the specimens to be photographed. Where this is not advisable, the head of the pin should be cut off and the cut end blackened with India ink. The pin can be almost eliminated in a photograph if its axis is arranged to coincide with the axis of vision. Specimens to be photographed should be cleaned of any particles of dust or lint by means of a small camel's hair brush.

Illumination.—Various radiants may be used for illuminating the object to be photographed, namely,—sun, calcium, electric, gas, or kerosene light, named in the order of their actinic power. In using daylight, north or east light is preferable for photographic work of this kind. Specimens photographed in direct sunlight frequently cause reflections and halation on the sensitive plate, and hence it is not so satisfactory as a more subdued constant light. To secure even illumination on all sides of an object where the light is admitted at but one side of the room, the entire apparatus may be suspended so that it may be revolved during the exposure of the plate. Special light may be thrown upon any portion or all of an object by means of a concave mirror such as is provided with the ordinary compound microscope; or a paraboloid lens may be used to concentrate the light. A softer light reflected from a half cylinder of white paper is often more desirable.

Background.—Faulty and imperfect backgrounds ruin the appearance of more amateur pictures than any other of the minor defects. If the glass plate mentioned above is always used, with the background either white or black, at some distance below, perfect negatives without shadows may be secured. The best black background can be made by lining a deep box with black velvet, or dull black paper such as is used in packing photographic plates. Covers to the box with orifices of various sizes to suit the specimens add to the intense blackness of its interior. In case a black background is used great care in arranging the specimens on the glass plate is necessary to prevent accumulation of bits of dust or lint. Such particles of dust or lint are strongly reproduced on the black background, though scarcely visible on a white background.

Plates.—Any of the standard plates are good for this work, but special brands are preferred by some. Seed's Gilt Edge No. 27, a very rapid plate costing a few cents more per dozen than others, has given complete satisfaction. A slow plate may be preferable in many cases, but where time is a factor a fast plate is a time saver, especially in photomicrography, where exposures of several minutes are often necessary. Orthochromatic, color corrected and non-halation double coated plates often give better results than the ordinary single coated plates. It should be remembered that one-half strength developer should be used on double coated plates.

Developer.—A most satisfactory single solution non-staining plate and film developer which will keep indefinitely in stoppered bottles. and may also be used for developing Velox and kindred papers, can

be made in any quantity, using the following proportions, by thoroughly dissolving in water in the order given.

1 oz. Metol.
2 oz. Hydrochinon.
16 oz. Sulphite of Soda.
14 oz. Carbonate of Soda.
60 grains Potassium Bromide.
320 oz. Water (distilled or boiled).

Directions for use.—For Velox and kindred papers, full strength; for ordinary plates, stock solution 1 part, water 1 part; for films and double coated plates, stock solution 1 part, water 2 parts.

Operations.—One of the most important operations of photography is the proper focusing of the image on the ground glass. At high magnification the image is so obscure that it is often necessary to use a focusing glass. A focusing glass may be purchased, or it is comparatively simple to make one from an ordinary dissecting lens mounted in a cylinder of proper length, according to the focal distance of the lens. This focusing lens is placed on the ground glass and enables one to secure much greater exactness than is possible with the naked eye. After the object is properly focused on the surface nearest to the camera, by stopping down the lens it will be seen that the depth of focus is increased wonderfully as the diaphragm orifice is reduced, thus bringing all parts of the convex insect into sharp focus. If the lens is focused on the portion of the insect farthest from the camera, stopping down the lens will not bring the nearer portions into sharp focus, as objects beyond the original point in focus only, are brought into sharp focus by stopping down the lens. It must be remembered that in judging the time of exposure the stop at which the lens is placed and the bellows length all contribute to variation in the time. As one enlarges from natural size, each full diameter enlargement requires double the amount of exposure required at the previous enlargement. With the stopping of the lens the time of exposure is almost squared.

It is often desirable to make two or three, or even four, exposures of an insect from different points of view on the same plate, such as dorsal, lateral, ventral views, etc. This seems at first a rather difficult proposition, but by the use of two diaphragms, each covering one-half of the photographic plate, dividing the surface in half lengthwise and crosswise, any four separate exposures may be made on the same plate by simply shifting these half diaphragms. These diaphragms may be made of black cardboard, or better still, of hard rubber plates, such as are used in an ordinary plate holder. These

can be cut to fit in the end of the bellows nearest to the plate, but never to be used in the plate holder.

In case one is using the ordinary photomicrographic stand it is very convenient, by means of two millimeter rules, to ascertain and mark upon a stick just the exact bellows length required for various enlargements for the various lenses at hand. In this way much time will be saved, since the length of bellows for any desired enlargement can be readily determined.

Photomicrography

The same photomicrographic stand mentioned above is available for this work, but no special lenses excepting those furnished with the compound microscope are absolutely necessary. The objectives and oculars furnished with the microscopes of years ago were not properly corrected for photomicrographic work, but very good work can be done with the lenses of very recent years without purchasing special photomicrographic lenses. The object to be photographed should first be focused through the microscope in the ordinary manner, and the lower diaphragm should be stopped down to the light which is best for ordinary examination of the specimen. The microscope is then placed beneath the bellows and the hood attached to the tube, and by shortening or lengthening the bellows, the image can be arranged at the proper size on the ground glass. The amount of light passing through the high power lenses is so slight that a focusing glass is absolutely necessary to see the image. It is frequently necessary to refocus the microscope so as to bring the object into sharp outline on the ground glass. Then by stopping down the lower diaphragm slightly and reducing the light, a greater depth of focus can be obtained, although requiring much longer exposure to obtain a good negative. In order to obtain the greatest depth of focus possible, it is advisable to use the lowest power objective which is suitable, and a long bellows rather than a short bellows and a high power objective for the same picture.

Better results can frequently be obtained by the use of stained specimens. The stain should be one of the colors which will reproduce in black on the sensitive plate, the red tints being preferable. The plate which has been exposed for a photomicrograph should be developed rather slowly and carried to the limit, being careful lest in overdeveloping detail may be lost.

A MEMBER: I wish to ask how you avoid reflections in alcoholic specimens from the blacks.

MR. SANDERS: I use a double coated non-halation plate. This larva which I have photographed was in a shallow glass dish with no glass between it and the lens.

A MEMBER: What is your enlarging apparatus?

MR. SANDERS: Any ordinary camera lens can be used. The bromide paper is an exceedingly fast paper, faster than Velox, perhaps a hundred times, but is printed and handled in the same way.

MR. PARROTT: What paper do you use?

MR. SANDERS: I have forgotten the name of the brand but any bromide paper is satisfactory.

CHAIRMAN BRITTON: The next paper on our program is "Photomicrography of the Diaspinæ," by Mr. R. A. Cooley.

An abstract of the paper was given at the meeting, the paper in full follows:

PHOTOMICROGRAPHY OF THE DIASPINAE

By R. A. COOLEY, *Bozeman, Montana.*

In these days when economic entomologists are frequently called upon to make scattering identifications in widely differing groups, it is desirable that systematic papers be written in unmistakable language and that the illustrations be such as to leave as little doubt as possible. The economic entomologist considers systematic work to be a means to his end and believes that systematic papers should be as generally usable as possible. The advantage of photographs over drawings in illustrating entomological publications is well recognized, though pen drawings of detail still and always will have a place.

Any one who has attempted to make pen drawings of the terminal segments of the Diaspinæ is aware of the difficulty of securing anything like a pictorial effect or even reliably representing the original. It has been found that good negatives can be made of these insects, particularly when the microscopical mounts are made with this purpose in mind. The following remarks, which are largely based on our experience in preparing a series of photographs of the genus *Phenacaspis,* will apply in many respects to the photomicrography of other similar objects.

Preparing the Mount.—The writer formerly employed glycerine jelly as a mounting medium for scale insects on account of the greater difference between the refracting qualities of the nearly transparent chitinous parts of the terminal segments and the glycerine jelly than exists between the chitin and balsam. The glycerine jelly mounts

were sealed with a ring of cementing substance like white zinc cement or gold size, but it was found that mounts that had been made a few years were badly damaged through the evaporation of the water in the glycerine jelly. It was necessary to remount all specimens preserved in this way. Canada balsam is now used and it is found that by attention to some details sufficient contrast can be secured.

Specimens to be photographed should be cleared and "posed" with unusual care, all foreign material, as dust and disintegrated tissues from within the body, being carefully excluded. The writer clears specimens by boiling in caustic potash solution in a small, shallow, porcelain dish, replacing from time to time, with a finger-bulb pipette, the water that boils away. The cleared specimen is taken up with a pipette and dropped into filtered water in a clean vessel. From the water it is passed through two or three graded alcohols which leave the empty body wall in an expanded condition. For the best results it is sometimes desirable to pass the specimen through two or three graded alcohol-clove oil mixtures. The specimen is taken up in a drop of the clove oil and placed on the slide and as much as possible of the oil is removed with small strips of bibulous paper. Care must be taken at this point not to introduce dust or threads from the paper. A small quantity of thin, filtered xylol-balsam is added and a number two, half-inch, circular cover-glass is applied. Exceptional care should be taken to secure a thin mount, thus making it easier to bring all parts of both the ventral and dorsal surfaces into focus under high magnification.

The Set Up.—Our first negatives were made with a home-made device consisting of a slotted board fastened in a vertical position to a low platform on which the microscope was placed. An extra lens-board in the front of the camera to which was attached a light-proof sleeve which dropped over the barrel of the microscope was found to be effective in making a light-proof connection between the camera and the microscope. This sleeve allowed sufficient play for focusing the microscope. Such a set-up can be secured with very little cost and if vibration is prevented as good negatives can be obtained as with a more expensive equipment. For convenience the writer now uses a heavy iron stand made by the Bausch & Lomb Optical Company which has two parellel guide rods with a scale on one for recording the degree of extension of the bellows. Our stand has extra long guide rods, made to our order for use in other work. This has been found to be very satisfactory and with the various conveniences attached, one can work much more rapidly. Nearly all of our insect and flower photography is done with this stand and our various

lenses are quickly attachable to a set of interchangeable lens-boards which fit this camera.

We have a complete series of Zeiss microscope objectives as well as various others, and find it a great convenience to have a long series, as we can always secure the degree of magnification and at the same time size of image that we require.

Magnification.—In our work on scale insects we have found it dedesirable to secure the required size of image by the use of a high power lens and short bellows rather than with a low power and longer bellows. This we have done for the sake of securing greater detail in the print. At the same time it is desirable to bring the whole object into focus if possible, and the higher the power used the less is the focal depth. One may, with a lower power and longer bellows, produce as large an image, but no more detail can be expected than is brought out by the power of lens being used.

In photographing thicker objects which present much detail, it is necesary to use a lower power and, if the length of bellows is not sufficient to produce the size of image desired, it is necessary to make enlarged copies from the negatives or prints.

Plates.—We have used several kinds of plates but prefer one made to our order by the M. A. Seed Company. This plate is their regular lantern slide emulsion on 4 by 5 glasses. It is difficult to estimate closely the proper exposure for scale insects of varying density and this plate allows us greater latitude in that respect and results in a saving of both time and plates.

Prints.—We have thought it desirable to dissolve out the background of our prints and obliterate parts of the image not required. This is done as the prints are removed from the fixing bath, by the use of a solution of hypo and potassium-ferri-cyanide. The print is placed, face up, on a piece of glass over a tray containing the solution. A swab of absorbent cotton is dipped in the solution and applied to the parts to be dissolved out. The glass is inclined so as to allow the solution to run away from the print and into the tray.

Regular glossy Velox paper is used when it is desired to obliterate parts of the print but, in general, Solio papers are to be preferred. The Velox prints are dried on a ferrotype plate as with Solio papers.

Chairman Britton: There is now a chance for a discussion of this paper.

Mr. Skinner: I wish to say that we have trouble in using the ordinary lantern slide plates. I would like to ask Professor Cooley

whether he has his plates treated with the emulsion and whether such plates are on the market?

Mr. Cooley: No; they are made by the M. A. Seed Company in gross lots.

Mr. Washburn: What power lenses do you use?

Mr. Cooley: A Zeiss D, I believe, was used on this series of photographs and they are all the same scale of magnification.

A Member: I would like to ask Mr. Cooley whether he thinks photographs will always bring out the surface markings that we can get sometimes by drawings. I think, with certain insects, like scale insects, we want to get an accurate representation of the insect as it appears.

Mr. Cooley: That is true. There are many kinds of illustrations in which drawing only will bring out what is desired. However, any one who has attempted to draw scale insects will realize that in the *Diaspinæ* it is very hard to bring out anything like what is wanted in a drawing.

A Member: I think the two things have been reversed recently. That is, the drawing was the primary thing in times gone by, and it ought to be relegated to the second place. That is to say, it should supplement the photograph, if necessary.

Mr. Felt: It seems to me that the making of photomicrographs is one of considerable importance, and I believe that quite a few have been deterred from attempting work along this line because of supposed difficulties. I want, for just a moment, to outline an apparatus which we have used at Albany, N. Y., with a good measure of success. We were situated so that we did not think it advisable to put a considerable amount of money into special lenses, or upright stands, or anything of that kind. We simply used the ordinary microscope, put it in front of a long bellows, and used a Welsbach lamp, and put a boy at one end to manipulate the slide and microscope, and a man at the other end, to give directions so far as adjustment was concerned. In that way, we were able to make a pretty fair series of photomicrographs. We could have made them better if we had had better equipment, but we found that in ordinary enlargements, fifteen, twenty, twenty-five or thirty diameters, we could make, with that make-shift apparatus, ten or twelve pretty good photomicrographs in an hour, with one man and a boy to attend the microscope. If you wish to get an idea of the work done by this outfit, I would simly refer you to Museum Bulletin No. 79 (N. Y.), illustrating some mosquitoes. I am satisfied that we can use ordinary lenses and work in the daytime. We all have practically that

outfit in the laboratory, and we can use it or not, and we don't have several hundred dollars laying idle in special apparatus, which is, perhaps, used only three or four days in the year.

CHAIRMAN BRITTON: If there is no further discussion, we will proceed with the next paper on the program, by Mr. F. M. Webster.

THE IMPORTANCE OF PROPER METHOD IN ENTOMOLOGICAL INVESTIGATIONS

By F. M. WEBSTER, *Washington, D. C.*

It is not my purpose to lay down a series of rules and regulations governing details in the work of any one; therefore the use of the word "method" instead of "technique." As a matter of fact, variety in insect life, and the conditions under which these must be studied, is so great that, beyond general principles, each investigator is forced to accept situations as he finds them and to make the utmost of his opportunities.

The spirit that prompted this paper came from the fact that the older entomologists of the country have, almost all of them, passed away, and those of us who, a quarter of a century ago, were young men are now also passing away, and a decade hence we shall nearly or quite all of us have practically given place to the younger men who now constitute by far the majority of the membership of this association. I thought that it might be possible for me to tell you of some of the things learned during nearly forty years' study of insects and insect problems, beginning at a time when entomologists were few, with no such training as is offered today available, and with precious few publications dealing with insect binomics. Strenuous days those, but in later years I have come to look back upon them with greater leniency and indeed feel thankful for them. Unfortunate indeed is the man who is denied the opportunity to show what his abilities really are, something that even he may not himself have learned.

With all of us who have the management of men there comes a time when an emergency arises and some one must be detailed to a most difficult piece of investigation, where only the most resourceful, persevering and trustworthy are to be employed, and lucky is the man who gets the opportunity. Now if I were to be asked to indicate some of the most essential qualifications for such a man I should say, first, honesty, and, second, common sense. Without these all of the training and equipment in Christendom will avail nothing. Some of us do not see as clearly or as broadly as others, it is true, which is of

course a personal misfortune, but not to be able to translate the truth, exactly as we see it, is a fatal defect. If there is a profession, the ministry not excepted, where clean men are more essential than in scientific research, it would be difficult to name it. Entomologists are not angels, and are not likely to become so in the future, in this world at any rate, but the man who goes after the truth for truth's sake is industrious, ingenious and persevering, will be about as sure of success as one day is sure to follow another. Incidentally, however, success does not consist of getting your name in the papers with the greatest frequency; the merest charlatan does that; nor does it mean exploiting yourselves before a few farmers who are seldom posted, for the untrustworthy have a monopoly in that direction. The man who beats the bass drum makes the most noise, but he is not the leader of the band. There is all the difference in the world between becoming famous and becoming notorious. You may secure fame or notoriety, but never both. A really good entomologist has no need of a placard advertising the fact, because in the process of his development he has been obliged to exhibit qualifications and combinations of qualifications that are never to be found in an inefficient man. No single qualification alone leads one to success, but one must needs be well balanced; be sufficiently pessimistic to be able to justly but fearlessly deal with defects in the work of others as well as his own, and yet be optimistic enough to see in advance sufficiently clear to formulate working theories and hypotheses without prematurely adopting these as fixed truths. Deficiency here is the reason for so many failures, and, often, too, failures that seem almost like the frivolities of chance. But it is not so. There is some defect, something lacking that is essential to success. Naturally the unfortunate himself will say it is due to a lack of opportunity or proper appreciation. Not every one is capable of grasping an opportunity when offered him, and as to appreciation, the world is not such an unjust judge after all, though it is sometimes very tardy in rendering a decision, and while the lines

"Seven cities fought for Homer dead,
While the living Homer begged for bread,"

finds a parallel perhaps all too frequently, yet such cases are, at most, not usual, and real success is, after all, in our leaving of the world better than we found it. Besides, it is often the fear of not being appreciated that drives men to make the mistake of chasing about after notoriety and in consequence accomplishing nothing meriting success. All of this leads me to make what may seem to you to be a remarkable

suggestion, and that is: Do not try to become famous or even noted except as a conscientious man. Get close to nature and throw the whole weight of every faculty that you possess into learning the truth unmixed with error, and get as near the whole truth as it is possible for one human to secure. Your province is to get facts sifted and tried by every clarifying process you can devise, with a view of eliminating obscurity and error. Pay absolutely no attention whatever to your own individual prominence, and as fast as you secure the real truths involved, just so fast will you gain the reputation of being something far above notorious. Success will not only come to you but be even begging you to take notice thereof. The old threadbare saying that "Some men are born great; others have greatness thrust upon them" is not true. Real greatness is never inherited; and the man who has real greatness thrust upon him has always bought it in some manner with the best years of his life. Substitute notoriety for greatness and I have nothing to say. The man in search of notoriety is generally the one that succeeds in getting in the way. In fact such is his usual mode of procedure in making himself notorious. For this reason the man who is trying with all his might and main to wrest from nature her most profound truths is obstructed by the one who, not knowing what truth and accuracy really are, will place himself squarely in opposition. Thus it is that the man who sets out to devote himself to dragging forth truth out of darkness or obscurity will find that his is not a bed of roses. Not only must he hold his own faculties under a continued surveillance lest he be cheated by his own eyes and mind, but he must always be more or less hampered by the frailties of those who find it easier to adopt other less commendable methods. Within the last two months there have come to my desk two publications from as many different states relating to two different though closely allied insects. In neither one is there anything new, nothing to indicate that either insect has ever been known outside the state where it is mentioned, and not a word to show that anyone, living or dead, except the author, had ever seen or studied the species. Quite recently one of the institutions from which one of these documents emanated published a newspaper bulletin, not necessarily by, or even with the knowledge of an entomologist, giving what purports to be the results of several years' experimentation, proving certain facts that were well known and established before that institution came into existence. Any well informed entomologist could readily cite the work of investigators, some of whom are still living and some dead, who contributed to the sum of this knowledge; but in this printed document to which I refer every

word is stamped with the mark of originality. Now such things as these have no relation to either scientific research or the diffusion of truth. They are, purely and simply, bids for the cheapest sort of notoriety, and will stand, if indeed they stand at all, in future as monuments of condemnation for whoever has been instrumental in their creation. The probabilities are, however, that they will be cast aside and disappear as of no value whatever and be eliminated precisely as any other impediment. After all, this whole matter of the separation of the truth from error and falsehood may be likened to a huge system of sifting screens, each screen perhaps representing a decade of time. All of the work of human intellect is thrown into a hopper and the mass begins to be shaken downward. Gradually mistakes, misstatements, jealousies, and all of the results of human frailties, the dross if you please, is discarded and finally at the bottom we have the net results of scientific research. Your success in life as investigators will depend not so much on the bulk that is thrown into the hopper and the noise and dust arising therefrom as upon how much of this pure fact at the bottom of the screens you can yourself lay claim to. I will show you in a moment how the identification may be made. This matter of proper credit as between superior and subordinate has several times been discussed by this body, always, as it seemed to me, with a too narrow view of the subject, making it something of a personal matter. I do not wish to discuss it in this way, as the question does not seem to me to be definitely understood.

Perhaps I can best approach the matter by offering an illustration. You may become estranged from your child; you may disinherit him, disown him and refuse to recognize him in every legal way. He may even be adopted by another. But still the blood of his father and mother alone will flow in his veins. Their ancestry will be his ancestry, and no law in the universe can make it otherwise. Now every original, unprejudiced observation made is the child of one mind and one pair of eyes, and these alone can be held responsible. There is here a certain entirely natural proprietorship that cannot be either stolen, given away or disowned, any more than a child can be disassociated from its ancestry. Whoever attempts to destroy this responsibility commits a crime, not against individuals, but against science itself, and no crime against science, which is only another term for truth, goes ultimately unpunished. Not only is this true with the original observation itself but all others following thereafter, whether contradictory or confirmatory.

When we come to the subject of publication, we are dealing with

the results of observation and investigation. The custom is, I believe, almost universal in all countries, which gives to the employing institution, for purposes of publication, all of the results of the labors of those in the employ of such institution. This seems entirely just, and there can hardly be serious criticism, so long as the observer is held responsible, given proper credit, and is not obliged to state what he does not believe to be true. There will probably always be occasional disregard of the rights of the observer, but these are not common and becoming less and less so every year. As the injury is not to individuals but to science itself, there is a certain losing of caste among those who do these things, which tends to prevent their occurrence. The question as to just when results should be published is a somewhat complicated one, and I sometimes wonder if the opinion of the investigator, even though a good one, is always the better. If we were to gather together all of the best of American investigators, I doubt if there would be one among them who would not recall instances where he had published prematurely and regretted it afterwards. To those who are in quest of notoriety, it of course does not matter. In my own experience, after working with a problem until there seemed no possibility of serious error, the results were printed and almost before the printers' ink was dry there would be sudden and unexpected developments that would completely upset previous conclusions. Indeed, it is strange how frequently such things will happen, even with what seems proper caution and the best of intentions. The older I get the more it seems to me that just here the investigator should be able to hold himself well in hand. Many a basis for a good piece of work has been spoiled by rushing it half seasoned, as it were, into print. Just here, too, is a point that younger men are prone to overlook. So long as matter remains unpublished all revisions or corrections constitute presumable added perfections; but once the matter is printed all of these become criticisms and reflect on the accuracy and conservatism of the author, even though he may himself make the revisions. Then again, these revisions of published papers may not in every case reach every one of those who have received the original and thus the misinformation at first diffused may not all of it be overtaken and rectified. While, then, admitting that there are cases where the matter of publication is in the hands of those who do not know what really constitutes an investigation and are mentally unfitted for judging the value of results, yet it seems to me that these instances are exceptional and that the prevailing custom is with these exceptions as good as we can at present devise.

There is, however, another feature of the question that should not be lost sight of, viz., one's duty to his colleagues. By this I mean he has not, morally, the right to horde up what he learns and thus prevent its being placed on record where other investigators may profit by the information. But, it may be suggested, this will flood our literature with unfinished work. It will inject into our entomological publications the many fragments of information that all good observers will and must accumulate in carrying on any other larger investigation; and the publication of these places them at the service of other workers. Possibly there may be one who is engaged on a kindred problem in another part of the world but who finds himself practically at a standstill for the lack of one of those fragments of science which he is unable to secure,—the key stone to the arch, as it were. Besides, we must remember that no entomological work is ever completed. The best that can be done only remains the best until some one with improved facilities and technique, or with added biological knowledge, shall be able to do better. I sometimes liken this to an endless stairway, with baskets placed on each step. The stair is progress and the basket the problem. An investigator, as he makes his way forward and upward, takes up a basket and carries it forward one or more steps, then sets it down and passes on into the unknown. Another follows and perhaps does the same, and in this way is advancement accomplished. I never take up a problem that was begun by Harris, Fitch, Riley, Lintner or others without a mental picture of the stairway and baskets coming to my mind's eye, and with a feeling of reverence for the good that these men, working with crude instruments, primitive technique and almost no literature at all, were able to accomplish, and wonder how far I shall be able to lift and carry the basket onward and upward before, like these men, passing to the great unknown beyond.

I have referred to a more perfect equipment because all of our instruments used in scientific research are continually being improved. Also I have referred to a wider and deeper knowledge of binomics. This leads me to say what you already must have remarked, that economic entomology today is not at all what it was ten years ago and it will not ten years hence be what it is today. If I mistake not, upon you younger men will devolve the duty of making many and diverse revisions. Our present system of classification will frequently be found wholly inadequate for your necessities, and our laws of priority are too ridiculous to stand except as an element of discord.

In dealing with the work of the systematist, I first wish to call at-

tention to the fact that this work has been, almost without exception, a labor of love, carried on by men with no thought of monetary compensation, and in the midst of lives exceptionally greatly preoccupied with human affairs around them. They did their best, considering the difficulties under which they were obliged to labor, and the monuments to their self-sacrifice and zeal are not to be ruthlessly torn down and obliterated. But we shall here have a case parallel with the old, low, weather-beaten, historic building giving place to the modern structure of concrete and steel. You to whom will fall the duty of this revision will need to look well to it that you leave these things more advanced than you found them. If I mistake not the time will come when no one will be allowed to describe a new species or revise a group of old ones without being able to present also something in the way of descriptions and explanations of the developmental stages or studies of habits, going to show, beyond a reasonable doubt, that the forms with which he has dealt are really what he represents them to be. So long as the science of entomology consisted in the collection and arrangement of dried corpses the system of classification in vogue was sufficient. But with the new era of entomological research, where insect binomics and the interrelations of different species are more and more generally and fully entered into, the structure is too frail and defective, so that almost as soon as we begin to build upon it we find it full of defects and inevitably it must be discarded and reconstructed on a much broader basis. These are problems that will be forced upon you and which will not for a moment permit slovenly or inefficient work. Please let me explain the use of this last sentence. In some quarters university people seem to be confirmed in the opinion that a graduating student can only make himself and his *alma mater* famous by describing something. It does not seem to matter much what, but once he has done this he has almost smothered himself and his university in a brain storm of glory; sometimes to the discouragement of the poor fellow, who, fortunately, is unable to comprehend the desirability of such proceedings; and I hope to have said enough here to sustain the latter in his stupidity.

Some of you may wish to remind me that a few moments ago I said something about the laws of priority. Only a single instance out of many will be required to illustrate the point that was made. *Smerinthus geminatus*, a common sphingid moth, was described by Say in 1824. It is a common, somewhat variable, species, the specific name, *geminatus*, having reference to the two ocelli on each of the posterior wings. In 1773 Drury described a single moth as from

Jamaica, figuring it with a single ocellus, giving it the specific name *jamaicensis*. Now in rearing a great number of adults from the egg an occasional individual of this latter form will appear, and all gradations between it and the true *geminatus* have been repeatedly observed, and may be reared from the same lot of eggs. The species has never been since found in Jamaica, and no one now believes for a moment that it ever occurred there, Drury's specimen probably having been mislabeled. Neither description nor figure represents the species but an occasionally occurring variation, and the name is not only a serious misnomer but misleading as well as false. Yet, on account of priority of publication, if the laws of priority are followed, this must be considered as the species. This is one of the things that must either be put out of the way or allowed to stand as perpetual contradiction and a discredit to the science of entomology. In the Fifth Report of the U. S. Entomological Commission, pp. 601–602, is a footnote by the late Dr. C. V. Riley, which reads as follows:

"The law of priority becomes a nuisance and a positive injury to the science when pushed to the unnecessary extreme of attempting to solve inexplicable riddles."

I have in the foregoing pointed out some of your frailties, for the man who makes no mistakes is yet to be born, and indicated some of the problems that the older entomologists will probably bequeath you for solution. Besides these, during the years to come, many biological problems reaching far beyond the realms of entomology will be solved by closer, broader, and more careful studies of insects and insect development. Never in the history of American entomology has there been so much to do, and a greater demand for the right kind of men to do it.

Those among you will succeed who adhere closest to nature, who throw all of the weight of every faculty that you can command into your work, with an eye to bringing out the truth for truth's sake, and not for whatever temporary glory or notoriety there may be in it, remembering always that it is not the bulk that you throw into the hopper but what remains and is not rejected through the siftings of years that will stand to your credit long after you have yourselves passed away.

(During the reading of the above paper, President Forbes entered and assumed the chair.)

President Forbes: Discussion of this paper is now in order.

Mr. Marlatt: Mr. Chairman, I am very much pleased with and interested in the very thoughtful paper presented by one of the oldest of our members, and I wish to endorse with much heartiness his advice and suggestions. The field of entomology has increased enormously, the number of workers are a hundred times what they were not so many years ago and the amount of money expended has increased at a similar rate. When I entered the service of the Department of Agriculture the lump appropriation was twenty thousand dollars. Our appropriation now runs up to nearly half a million and the same relative increase has been seen throughout the country. With all this increase in entomological workers and funds and facilities, there has been a notable sub-division of the work. All the old problems that the pioneer entomologists attempted to solve are now attacked with a minuteness and specialization that was not then possible. The result is that as this old work is gone over the information is vastly increased, and errors are being constantly found. That is as it should be. The only point I wish to make is, that here and there you find in this new work a spirit of rather sharp criticism of the mistakes of the elders. It seems to me that this attitude is quite unnecessary. We should be charitable and remember that in those old days one man covered the field that is now covered by a score. Take, as an example, the white fly work in Florida, which has now been the subject of three years of continuous investigation by three men. Necessarily, they will find some of the older work faulty, and they will make large additions, but if the spirit of generosity and kindness prevails in these new workers, there need be no unpleasant or sharp criticism in the corrections which they necessarily make.

Looking over the work that is being put out today, the best work is by men who are most courteous and who have least in their writings that is unpleasantly critical. I think that credit should invariably be given. It is not necessary to fill pages with references to the writings of others. Credit can be given without interfering with the reputation of the writer in his own constituency and vastly increasing his reputation and standing in the field as a whole. I think entomologists may take a lesson from some of the other workers in science. There has been a good deal of controversial writing in entomology and bitter enmities have arisen through unnecessarily sharp criticism, through failure to approach one another in a spirit of friendliness and courtesy. I have quite an intimate acquaintance with chemists, and I think, as a body, the chemists are more closely knit. They get more fun out of their meetings; and they seem to have a more friendly spirit toward each other than that sometimes exhibited

by biologists. It seems to me that they, among the scientific men that I have known, have maintained that spirit of generosity, and friendliness, and courtesy which makes their meetings pleasanter and frees their publications from unnecessary criticism. Of course, there are exceptions, and the entomologists are perhaps less faulty in these particulars than others, but it seems to me that this point in Mr. Webster's address should be emphasized. I think we should endeavor to develop the spirit of leniency and courtesy rather than the reverse.

President Forbes: If there is no further discussion we will now take up the next paper.

BIOLOGICAL NOTES ON MURGANTIA HISTRIONICA HAHN.

By R. I. Smith, *Raleigh, N. C.*

As a result of a somewhat disconnected study of *Murgantia histrionica,* I have ascertained a few points concerning the life history of this insect, which will be briefly presented, with the preliminary explantion that some of the work reported upon is not complete in many respects and may, by some of my entomological friends, be considered premature. However, it is hoped that the notes will be of some value as an addition to the knowledge of this troublesome pest.

Definite observations and notes were first made on April 4, 1908, at West Raleigh, N. C., where all the work herein recorded was done. On the date mentioned adult insects were found in abundance on turnip and collard plants, on which they were feeding and mating most actively. It was then observed that very few egg masses could be found and no nymphs were observed until ten days later, or on April 14. Hence this date is considered as approximately the beginning of the first seasonal brood for the year. Some springs may bring forth adults and young at an earlier date.

Notes on Egg Laying Habits

It is generally understood that twelve is the normal number of eggs deposited in each mass, and that these are ordinarily placed on end in two parallel rows of six each, closely cemented together, the eggs usually alternating like the cells of a honeycomb. As a matter of fact, more eggs are laid in more or less irregular masses than in two parallel rows of six each. Out of 94 egg masses laid by females under observation in the laboratory, 62 were irregular in form but contained 12 eggs each, and only 19 were regular, with 12 eggs each, while 13 masses varied in numbers from 8 to 14. Twelve eggs for each mass

is undoubtedly the average number, as evidenced by the fact that three females laid 11 eggs once and 13 eggs in the following mass, while another laid 10 and then 14 eggs. In all instances observed where less than 11 eggs were laid at one time, or where 11 eggs were deposited two or three times in succession, the female died shortly afterward.

The table giving the egg-laying record shows how often the egg masses are deposited, and it will be noticed that this time varies from two to fifteen days, but an average of about four days.

Concerning the time consumed by a female in depositing the eggs, a marked regularity exists. I have watched several egg masses deposited and find it takes about thirty minutes to deposit twelve eggs. A quotation from notes made at the time will serve as an illustration.

"April 15, 1908, 4 p. m. I have just watched and timed female of Pair No. 13 deposit a mass of twelve eggs. At 2.50 p. m. she was observed in the position assumed when about to commence deposition. She was apparently straining the abdominal muscles and moved the abdomen up and down occasionally, during which process she frequently stroked the tip of her abdomen with either hind leg. During this process a drop of moisture appeared, probably to serve as a glue for the first egg. At 2.56 p. m. the first egg was dropped, and the remaining number, making a regular mass of twelve in all, were deposited from 2.56 until 3.25, when the last egg appeared, or exactly twenty-nine minutes after dropping the first egg. Counting the six minutes that she remained in position preparatory to laying the first egg it required thirty-five minutes for the whole process. The time elapsing between the appearance of each egg is almost exactly 2½ minutes." If I had ten minutes before having to start for a train I would be willing to depend on measuring that time by watching a terrapin bug lay four eggs. Other observations have been made that verify this statement.

Incubation of Eggs

The incubation period varies greatly with the temperature. Eggs deposited from April 9 to 15 required an average of eleven days, some requiring twelve days, while from May 12 to 21 the average was about six days. In hot summer weather they may hatch on the fourth day.

Egg Laying Record of Hibernated Individuals

As already stated these observations were commenced on April 4th, when a very few eggs were present in the fields. Hence, of the fourteen pairs selected for the egg-laying record some may have deposited one mass of eggs in the field. For this record pairs were taken and

confined in large glass vials, with suitable food and a strip of blotting paper to absorb surplus moisture. It is my opinion that too little absorbent was used, the lack of which caused the premature death of some individuals. Six pairs were taken on April 4th, and eight pairs on April 7th. They were given fresh food (collard or cabbage) each day and the number of eggs counted and removed each morning. As the eggs are laid during the daytime the record is always one day behind as regards the actual date when the eggs were laid.

The shape of the egg mass, whether regular or irregular, and number of eggs are represented in vertical column, the letter "R" meaning regular, that is, two uniform parallel rows ("12-R" meaning 12 eggs in two rows of six each) and "I" meaning irregular ("12-I" meaning 12 eggs in an irregular mass).

DAILY RECORD OF 14 FEMALES OF THE HIBERNATED GENERATION.[1]

Date.	1	2	3	4	5	6	7	8	9	10	11	12	13	14
1908. April 7	12-I	12-R	12-R	......	12-I	......	......	......	......	......	......	......	......	......
8	......	......	......	12-I	......	12-I	......	......	......	......	......	......	......	......
9	......	......	12-I	......	......	12-I	......	12-R	12-I	12-I	......	......	......	......
10	12-R	12-I	......	......	12-I	......	......	......	......	......	......	......	......	12-I
11	......	......	12-I	......	......	......	12-I	......	......	12-I	......	12-R	12-R	......
12	......	......	......	11-I	......	......	......	......	......	......	12-I	......	......	......
13	12-R	......	......	......	......	......	......	12-R	12-I	......	......	......	......	......
14	......	12-I	12-I	......	12-I	11-R	......	......	......	12-I	......	......	......	12-I
16	......	......	......	13-R	......	......	12-I	......	12-I	......	12-I	12-I	12-R	12-I
17	......	......	......	......	......	......	......	......	......	12-I	......	......	......	......
19	......	......	12-I	......	......	......	......	12-I	......	......	......	......	......	......
20	12-I	12-I	......	......	12-I	11-R	......	......	......	......	......	12-I	......	......
21	......	......	......	12-R	......	......	12-I	......	12-I	......	12-R	......	......	12-I
22	12-I	......	......	......	......	......	......	12-R	......	......	......	......	......	......
23	......	......	12-I	......	12-I	......	......	......	12-I	......	......	12-I	......	Died
24	......	......	......	......	......	......	......	......	......	......	......	......	12-I	......
25	......	12-I	......	......	......	12-R	......	......	......	12-I	12-R	Died	......	......
26	......	......	......	12-I	......	......	......	......	12-I	......	......	......	Died	......
27	12-I	......	12-I	......	12-I	11-R	10-R	11-I	......	......	......	......	......	......
28	......	5	Died	......	......	......	......	......	......	4	......	......	......	......
29	......	3		......	......	......	......	13-I	......	Died	Died	......	......	......
30	......	......	......	......	......	......	......	Died	12-R	......	......	......	......	......
May 1	12-I	......	......	8-R	......	......	......	......	......	......	......	......	......	......
2	......	......	......	Died	12-I	......	......	......	......	......	......	......	......	......
3	......	......	......	......	......	Died	......	......	......	......	......	......	......	......
5	......	2	......	......	......	......	......	......	......	......	......	......	......	......

DAILY RECORD OF 14 FEMALES OF THE HIBERNATED GENERATION.[1]—*Concluded.*

Date.	1	2	3	4	5	6	7	8	9	10	11	12	13	14
1908. May 7		Died					14-I							
10									12-I					
12	12-I				12-R									
14									11-R					
15	11-R													
16					12-I				Died					
18							12-R							
19	12-I				12-I									
20							12-I							
22	12-I													
23					12-I									
25	Died						12-I							
27							12-I							
28					12-I									
29							12-R							
31					12-I									
June 3							Died							
4					11-I									
9					12-I									
11					Died									
Total egg masses	11	8	7	6	15	6	10	6	9	6	4	4	3	4
Tot. eggs	131	70	84	68	179	69	120	72	107	64	48	48	36	48

[1] Dates accompanied by no record were omitted from the original table.

This record indicates that both the length of life and number of eggs deposited varies considerably, but as a matter of fact, I believe that those females that died before depositing at least six egg masses would have lived longer under more favorable conditions. At the same time Nos. 2, 4, 8 and 10, females that deposited eggs quite irregularly toward the last, may very probably have died a natural death. Hence it would seem to me that by taking the average of the number of eggs obtained from females depositing at least six egg masses, we would have a fair average number that might be laid under normal or natural conditions. Granting this to be a correct supposition, we have 99 1-9 for the average number of eggs laid by each hibernating female.

Egg Laying Record of Second Seasonal Generation

To serve as a check on the egg laying record of the hibernated generation and to determine which, if either, lays the greatest number of

eggs during their life time, an attempt was made to get a similar record for individuals of the second seasonal generation. For this experiment nymphs in the last stage of development were collected from the field, taken to the laboratory, reared to adults and from them, on August 10, 1908, fourteen pairs were selected and isolated in 4-oz. bottles with suitable food. Unfortunately the mortality among these specimens was very high, due, I am now convinced, to an excess of moisture accumulating in the bottles. Consequently the record is unsatisfactory, but at the same time rather interesting.

Egg Laying Record of Fourteen Females of Second Generation

Out of this number separated on August 10 seven died between August 24 and 27, after laying from 12 to 36 eggs each. Three more died August 31, September 4 and September 7, each having laid 36 eggs. On September 21 another female died at 44 days of age from maturity, after laying 4 egg masses containing 46 eggs in all.

There remained only three females out of the original fourteen and as they laid a larger number of eggs and lived so much longer, their individual record seems to be of particular interest and is as follows:

The female of Pair No. 8 became adult on August 7 or 8, lived until October 5, or 58 days, and deposited 72 eggs, or a mass of 12 eggs each on the following dates: August 20 and 24, September 8, 17, 21 and 26.

The female of Pair No. 9 became adult August 8, lived until October 10, or 63 days, and deposited 71 eggs, or a mass of 12 eggs each,—except once, when only 11 eggs were found,—on the following dates: August 20 and 29, September 2, 10 (11 eggs), 17 and 21.

The female of Pair No. 10 became adult August 7 or 8, lived under observation until October 10, when she escaped, after depositing 84 eggs or 7 masses of 12 eggs each on the following dates: August 20 and 29, September 7, 11, 20, 25 and 29.

As already stated, it seems probable that the females that died so soon after maturity were killed by excessive moisture in the vials. In other words, the moisture caused them to adhere to the sides of the bottles, where they died in struggles to loosen themselves. As this experiment was followed closely, I feel confident in concluding that the average number of eggs deposited by the second brood is six or seven masses of 12 eggs each, or from 72 to 84 eggs. This is less than the average for the hibernating brood.

Concerning the Number of Generations Annually

In my opinion there are only three full generations in the vicinity of Raleigh. Some entomologists may be as surprised to hear this statement as I was to discover the fact. Owing to the numbers of nymphs present in the fields during November a fourth or fifth generation might be expected. However, there is a very strong argument against that being the case. I was unable to secure eggs from any terrapin bugs becoming adult later than September 1. A considerable number were reared in the laboratory and kept under observation to determine this fact. Since we know that adult females may live and deposit eggs for more than two full months, the presence of young nymphs in the field even as late as early December may be accounted for, and they may be classed as belated individuals of the third generation.

The statement that there are only three broods cannot, unfortunately, be absolutely proven because I was unable to follow the development during the summer vacation. However, the statement is deduced from the fact that the first generation at Raleigh, that is the earliest maturing individuals, became adult on May 25, and deposited eggs nine days later, or on June 4. These eggs hatched on June 8. I found therefore that the first generation required fifty-one days, not including eggs, from April 14 to June 4, to become full grown and deposit eggs, and subsequent rearing experiments do not show much more rapid development during the hot months of summer. Furthermore, May 25 is the date of maturity of the first individuals of the first generation, while the majority were not mature until at least ten days, or two weeks later. It may be true that some bugs of the third generation mature before September 1 and commence to lay eggs, but I feel convinced that the majority of the adults of this generation do not lay eggs, but live over winter as the hibernating form.

Length of Life Cycle

There is some variation in the duration of the young stages, particularly the 4th and 5th instars. Chittenden in Bulletin 103, Bureau of Entomology, records the life cycle for bugs hatching in March as 70 days, including the egg stage, which covered 11 days. My record of bugs that hatched on August 24 and 25 shows the life cycle, exclusive of the egg stage, covered from 57 to 65 days.

The following table shows the record of four individuals which were kept in the laboratory—not heated—during the time recorded:

LIFE CYCLE OF FOUR HARLEQUIN BUGS

No.	Hatched.	End of 1st Instar.	End of 2d Instar.	End of 3d Instar.	End of 4th Instar.	End of 5th Instar. Becoming adult.	Total No. Days.
	1908						
1	Aug. 25	Aug. 30-31*	Sept. 7-8*	Sept. 15	Sept. 27	Oct. 23	59
2	" 25	" 30-31*	" 7	" 15	Oct. 2	" 29	65
3	" 24	" 30	" 7-8*	" 16-17*	Sept. 26	" 20	57
4	" 24	" 30	" 7-8*	" 16-17*	" 30	" 27	64

* Indicates that molt occurred some time between 5 p. m. and the following 8.30 a. m.

As evidence that the duration of the 4th and 5th instars may vary it is only necessary to study the above table, and as further proof compare these dates with the time recorded by Chittenden, as above mentioned. He states: "The first or egg stage covered 11 days. The time from the hatching of the eggs until the first molt gave the first larval instar or nymph period, 7 days; the second instar required 13 days; the third, 8 days; the fourth, 14 days, while the fifth or pupal instar covered 17 days, a total of 70 days or 10 weeks . . . "

Means of Suppression

From an economic standpoint this work indicates that Harlequin bugs should be fought vigorously in the fall, particularly after September 1st, in order to prevent the young forms from maturing and going into hibernating quarters. It also serves to emphasize the fact that the adults first appearing in spring should be collected or otherwise destroyed before they commence egg deposition. I am convinced that the hibernating adults are more prolific than the succeeding generations, which makes it most advisable to destroy them late in fall or early in spring.

Parasites succeed in destroying a large per cent. of the eggs, but in all my work this season with nymphs and adults no parasites were secured from them.

PRESIDENT FORBES: The next on the program are papers on apiculture by Mr. E. F. Phillips and Mr. B. N. Gates.

MEANS WHEREBY THE ECONOMIC ENTOMOLOGIST CAN ADVANCE APICULTURE[1]

By E. F. PHILLIPS, *Washington, D. C.*

Bee keeping in the United States is progressing and considerable advances are being made in various phases of the work, but there still remains much to do before apiculture as an industry takes its rightful place as a phase of scientific agriculture. The honey resources of the United States are very great. At present vast quantities of nectar are wasted every year because of the lack of bees to gather it, and, even where bees are kept, the wastefulness due to crude and improper manipulation is entirely too great. Persons interested in the advancement of this industry fully realize this condition of affairs, but the difficulty is to set in order machinery whereby these conditions may be overcome.

Apiculture is a branch of economic entomology and should be recognized as such. It is true that the nature of the work is quite unlike that of most of the work in economic entomology in that the object is to propagate rather than to destroy. The difference is not so great, however, when it is recognized that apiculture is primarily a study in life history but of a very detailed nature.

At present apiculture is advancing somewhat slowly and for two reasons. The main obstacle is bee disease, which causes a great annual loss to the bee keepers of the country and prevents the enlargement of operations. The second obstacle and the one of which I wish to speak particularly, is the need of competent and comprehensive research and educational work among those interested in bees.

In speaking of the work which the economic entomologist may do in helping this industry, there are several points which may be mentioned at once to forestall any presentation of them as objections. The economic entomologist is a very busy man and work on bee keeping would be adding to his already heavy work. The amount of money available for a new line of work is usually small or possibly entirely lacking and, in most cases, work on bee keeping would be a new line of work. The training for work on bee keeping is not usually included in that furnished a prospective economic entomologist and it is a safe assumption that the practical manipulation of bees would be new work to most men engaged in state work. It therefore follows that any work which may be undertaken, in most cases at least, must

[1] This paper was read by title at the meeting, but as it is closely related to the one which follows, it is inserted at this point in the report.—SECRETARY.

be such as to require little time and money and a rather meager knowledge of practical bee keeping.

In a recent paper[1] I discussed the need and possibilities of apiculture and classified the needs as scientific, economic and educational. The scientific needs are those resulting from a lack of proper research along statistical, zoölogical, botanical, chemical and bacteriological lines. There is no real line of demarkation between scientific and economic needs, since economic work is but the application of the results of scientific work. The educational work is the presentation of the results of scientific and economic research to the man who needs it. In part of these lines of work at least the economic entomologist may be of the greatest help.

The furtherance of the work depends largely on a definite knowledge of the present conditions of bee keeping, and to this end the Bureau of Entomology has undertaken the compilation of statistics as to the conditions of bee keeping in Massachusetts. Of the technique used in this work, Mr. Gates will speak more fully. Similar work is being undertaken in Maryland in coöperation with the State Entomologist. The work in Massachusetts has already resulted in great good in stirring the bee keepers to greater activity, and they are now actively engaged in getting a disease inspection law passed. In Maryland, with the work just begun, a good bee keepers' organization has been established as a direct result of the work. This work is too large an undertaking for the Bureau to undertake in all the states, nor does it seem desirable, for this is work which seems more properly to belong to the state. The advantage of a detailed knowledge of the present status of the industry, with information as to the honey resources, is of inestimable value in planning educational work. This work involves considerable time and may, therefore, be out of the question for many state entomologists.

There are, however, several lines of work which suggest themselves. In a paper which I read before this association a year ago,[2] I urged that the state entomologists interest themselves in bee disease work. There is no other line of work in most states that would be of greater help to the industry. The distribution of bee diseases in the state is undoubtedly the best argument which can be used in asking for new laws and where laws already exist this should be definitely determined as an aid to the inspector and as an educational measure. This the

[1]Phillips, E. F., 1909. The Status of Apiculture in the United States, Bulletin No. 75, Part VI. Bureau of Entomology.

[2]Phillips, E. F., 1908. Bee Diseases: A Problem in Economic Entomology. Journal of Economic Entomology, Vol. 1, No. 2, pp. 102-105.

entomologist can do with little work. He can soon learn to diagnose American foul brood and most cases of European foul brood. In case of doubt, the Bureau of Entomology will be glad to make bacteriological examinations. The examination of samples not only is the means of identifying samples for bee keepers who do not know the diseases, but publications may be sent out or recommended giving methods of treatment.

It need not be pointed out that Farmers' Institutes are an important factor in agricultural education. The state entomologist may, without great effort, see to it that talks on bee keeping are included in the institutes, either by himself if he feels able to do it or by some good practical bee keeper. There is an element of danger in this work which should be mentioned. Bee keeping is not an industry in which every one can engage with profit and Farmers' Institute talks should not be of such a nature as to induce great numbers to take up the work. They should aim to make better bee keepers of those now in the business and encourage only careful persons to begin it. Any other procedure will lead to grave disappointment. The state entomologist may well exercise a little supervision over these talks to eliminate wild nature-faking, which is too often a fault with bee talks.

In traveling about the state the entomologist often has opportunity to give out information on bees and to get information of value to other bee keepers. It would be better if he could give out information himself in person or in letters or bulletins, but, if not, he can put the bee keepers in touch with the Bureau of Entomology.

A METHOD OF SECURING APICULTURAL STATISTICS

By Burton N. Gates, *Washington, D. C.*

Until recently, the only source of statistical information on apiculture has been the Federal Census; but, unfortunately, this is found incomprehensive. It fails to strike to the root of the industry and to give fundamental information, as for instance, on the prevalence of bee diseases, prevailing methods and implements, progressiveness in certain sections of the country, enemies and like economic problems. Such things the one who is attempting to promote apiculture must know. They are also of interest to the bee keeper, who cannot be enlightened by the Census Reports. Moreover, there is considerable evidence that the data presented in the Census Reports does not adequately picture the importance and magnitude of the industry. The

report for 1900[1] distinctly states that "the statistics of agriculture do not include any data . . . relating to animal products or crops raised by persons who pursue some calling other than agriculture, but incidentally care for a tract of land too small to be called a farm." Any one who is acquainted with bee keeping, in the East particularly, readily sees that this one fact could vitiate the significance of the statistics.

The plan to be presented was first tried on a small scale on Worcester County, Massachusetts.[2] Since then it has been perfected and tested on the state as a whole by the Bureau of Entomology.[3] As evidence of the extent to which the work has already benefited bee keepers, it may be stated that whereas a year or more ago the majority of apiarists laughed at the possibility of bee diseases existing in Massachusetts, today they are united in an effort to procure legislative protection and the appointment of a bee disease inspector. Furthermore, there is news of two new bee keepers' organizations in western Massachusetts.

It should be emphasized that the method to be presented is not limited to apicultural investigations, but may be adapted and extended to the study of the status of any agricultural industry, crop or pest.

The statistics were gathered by a questionary method, operated under frank through the mails. The frank proved to be an exceedingly important factor in securing a high percentage of replies. Besides relieving the expense of postage, experience shows that there is an element of anthority or charm in the franked envelope, which doubtless brought replies from many who otherwise would not have responded. However, even without the frank, it has been demonstrated possible to secure much valuable information.

In beginning the work, it was first necessary to locate the bee keepers. A printed letter was mailed to every town clerk and to each Grange secretary in the state. The letter stated briefly the project and requested names and addresses of bee keepers. Replies from the town clerks were more prompt and complete than those from the secretaries. But even more satisfactory were the returns from a few postmasters who were written to because of a failure otherwise to locate bee keepers in their vicinity. Were it possible to work exclusively

[1] 12th Census, Vol. V, Parts 1 and 2, pp. xiii-xiv.

[2] Gates, Burton N., 1906. Status of Bee Keeping in Massachusetts in 1906. American Bee Keeper, Vol. XVII, pp. 79-81.

[3] Gates, Burton N., 1909. Bee Keeping in Massachusetts. Bulletin No. 75, Part VII, Bureau of Entomology. U. S. Dept. of Agriculture.

with the postmasters, the writer feels sure that this would be the most efficient and rapid course. .

As fast as the addresses were received they were transferred to cards for filing. Two kinds of cards were used;[1] one set served as an index to the bee keepers of the state, and were filed alphabetically according to name. The other set constituted the record file and were arranged geographically by county and town. In this way there was a double and automatic check on duplicating entries. In transferring the addresses both cards were struck off on the typewriter at one operation, which not alone saved time but made certain that one card bore the exact inscription of the other. Experience taught that the work of locating the bee keepers should be begun several months before sending out the questions to the bee keepers.

Each bee keeper thus located was sent a list of questions to answer and return. This was brief, worded so as to be impossible of misinterpretation, and answerable by yes or no, by figures, or by a few words, such as the name of a hive. The order of the questions corresponded with the order of the columns and spacing on the record card. This, with the brief answers, facilitated the recording of data. At the end there was a request for names and addresses of bee keepers. It also proved advisable to give a chance for "remarks," which brought out much information not prompted by the questions.

Printed at the head was a list of the available publications of the Bureau of Entomology relating to apiculture, together with directions for obtaining them. The bee keeper was permitted to check such free bulletins as he desired. In this there was an incentive to reply to the questions, acting similar to a "free premium" in advertising. Furthermore, as a result, hundreds of bulletins were placed directly in the hands of those who wanted and appreciated them, circumstances which do not always prevail with government and experiment station literature.

After two or three weeks, if no reply was received from the bee keeper, a duplicate copy, on the corner of which was stamped in red ink, "SECOND REQUEST. *An Immediate Reply Is Earnestly Requested,*" was sent out. This stirred up delinquents, amply paying for the time and labor. Finally from sixty to seventy per cent. of those listed in the files were heard from. The remainder were probably small, one or two hive bee keepers, and consequently did not materially affect the results.

[1]Cards used for the address file were about 3 x 5 inches (Library Bureau Standard). The record cards were about 5 x 8 inches. (Library Bureau Standard).

As fast as the returns came in they were arranged preparatory to recording by county, town and name of bee keeper, corresponding to the arrangement of the record file. Recording was done on the typewriter, in symbols and abbreviations. Thus "8 L" stood for eight frame Langstroth hive; "It" or "It.x" represented Italian bees or Italian hybrid; "E. F. B." designated European foul brood. If a bee keeper was reported "deceased" or "out of the business" it was found best to so mark his cards and retain them in the files.

Gradually as the work progressed, through the fullness and character of reports, representative apiarists were selected and designated "Informants." These served as correspondents, and in many cases were asked for details of certain interesting local conditions. In some instances informants assisted in obtaining replies from neighbors who were indifferent in responding. In three cases very valuable and detailed information was received on local nectar floras. The discovering of reliable correspondents was an exceedingly valuable feature of the work.

The results of the writer's study in Massachusetts will illustrate what is possible in a state where no definite knowledge of conditions was previously available. In the first place more than 2,100 bee keepers were located, which was several hundred more than recorded in the 1900 Census. Between sixty and seventy per cent. of these reported. The size of crops were obtained. It was possible to learn the prevailing types of hives and to what extent the old-fashioned box hive still exists. Foci of American and European foul brood were located. Lists of the most important honey plants were made up. The extent of the trade in queen bees and colonies of bees and the extent to which bees are used in cucumber greenhouses was ascertained. Some sections of the state were found more progressive than others. Such things are not afforded by the Federal Census.

Tuesday Afternoon Session, December 29, 1908

The meeting was called to order at 1.00 p. m., with President S. A. Forbes in the chair.

President Forbes: The first paper on the afternoon program will be presented by Mr. C. E. Hood.

TYPES OF CAGES FOUND USEFUL IN PARASITE WORK

By C. E. Hood, *Dallas, Texas*

In the breeding work connected with the boll weevil investigations the inability to carry through a large percentage of the weevils and parasites showed very plainly the inefficiency of the various types of cages which were then in use. It was therefore necessary to construct several new cages furnishing more nearly the natural conditions under which the various weevils and parasites live. The object of this paper is to give a brief description of these cages, their advantages, and some of the results already obtained from their use.

Most of the breeding work at Dallas, Texas, has been carried on out of doors in a remodelled hibernation cage. This consists of a frame of two by fours, measuring ten feet on each side and seven feet in height, the top and sides of which are covered with 14-mesh wire screen. A roof was built over this and shelves arranged for cages on three sides. With such a cage it was possible to produce more natural conditions of temperature and humidity than were possible before in the breeding room of the laboratory.

Indoor Breeding Cages

Of the various cages used, our five-section cage is the largest. This measures four and a half feet in length, ten inches in width and twelve inches deep. The bottom and ends are of wood, the top and back of 50 mesh wire gauze and the front of glass. This cage is divided into five sections by wooden partitions, each section being entirely separated from the others. The panes of glass in the front can be raised or lowered and serve as doors. Pieces of felt-edged weather stripping are used in the grooves in which the glass slides to insure tightness.

This has proven to be a very satisfactory breeding cage. One disadvantage, however, is that the insects attracted to the light collect on the glass and it is impossible to get them without disturbing or oftentimes crushing some of them. This difficulty has been overcome almost entirely by having the side opposite the glass made of wood instead of wire, with a small door in the center large enough to admit the hand. To secure isolation from ants and mites, nails are driven part way up into the bottom of the cage, one on each corner, and the heads are set in small cups of vaseline or axle grease.

For smaller lots of material the box cage has proven to be quite satisfactory. This is a wooden box of the style used by the California Board of Horticulture. It is ten inches long, six inches wide and six

inches deep. This box is fitted with two covers, the inner one glass, the outer one wood. With such an arrangement it is possible to examine the material without allowing the insects to escape. Three holes are bored in the front of the cage, one inch in diameter, and in these are placed glass tubes about four inches in length. Insects attracted to the light come out into the tubes, where they can be easily collected. A layer of sand on the bottom of the box makes conditions favorable for larvæ that enter the ground for pupation.

If it is desired to watch the larvæ that enter the ground they are put into the double tube cage. This consists of one tube inside of another, the inner tube having no bottom. Dirt is placed between the tubes and the outer tube is encased in black paper. Water placed in the middle tube passes through the dirt by capillarity, keeping the ground in a moist condition. This inner tube because of the light forces the larvæ to the outside of the outer tube, where they can be readily seen by removing the black paper.

Smaller lots of breeding material are kept in tumblers in which has been placed a layer of moist sand. The tops of the tumblers are covered with cheese cloth held in place by elastic bands.

In our parasite breeding work it is desirable to obtain correct data on the length of development of the various stages. In order to do this each specimen is isolated and so labelled that the data on the individual specimen can be referred to at any time. These specimens are isolated generally as larvæ or pupæ in glass tubes with cotton stoppers or in gelatine capsules.

The Dipterous parasites are somewhat harder to carry through to maturity because of the lack of proper conditions of moisture. This has been supplied by a new type of cage. It consists of a tray filled with about an inch of sand. Glass tubes without bottoms are placed in the sand in an upright position. By means of two of these tubes, which are filled with water, the sand near the bottom of the tray is kept wet, while the sand on the top is kept moist from this by capillarity. A single Dipterous larva is placed in each of the remaining tubes, the tops of which are closed with cotton stoppers.

Plant Cages

Perhaps the most interesting type of cage in use at the laboratory is the mica plant cage. This consists of a mica tube eight inches long and two and a half inches in diameter, such as are used as chimneys for gas lights. One end is closed with cheese cloth and on the other end a cuff of the same material is fitted so that the tube can be placed over the top of the plant and the loose end of the cuff tied tightly

around the stem of the plant. On the sides of the tubes are three small metal rings. Pieces of strong string or twine fastened to some support above the cage pass down and are secured to these rings. They then are continued downward and after being pulled taut are tied to the stem of the plant below, where the cuff is attached. In this way the strain caused by the swaying and blowing of the plant is not brought to bear on the cage but on the stem below the cuff and on the support above. On the side of the cage a small opening is made for the admission of parasites. It is closed by a mica slide or a cotton stopper. About three inches above the tube a circular piece of cardboard about eight inches in diameter is placed. This is to protect the cage from rain and also from the direct rays of the sun.

In using this cage for our parasite work, weevil-infested squares freshly punctured are tied to the top of the plant. The mica tube is then slipped on over them and the cuff is tied to the stem below. The twine is fastened to the support above and to the stem below the cuff, and lastly the parasites are admitted through the opening in the side.

Up to this time great difficulty had been experienced in perfecting a plant cage which did not sweat. In the use of this cage this fall under the most favorable conditions for sweating very little moisture collected in the tube. What little there was was soon taken up by the cheese cloth. This type of cage furnishes plenty of light and air, the parasites are abundantly supplied with nectar from the floral and other nectaries of the cotton plant for food and in every way they seem to feel perfectly at home.

Very few observations on the oviposition of Hymenopterous parasites, especially the Chalcidids, have been placed on record, but notwithstanding the fact that these cages were not put into use until about October 1st, notes on the oviposition of two species of Chalcidids and two species of Braconids have been made.

Another plant cage for similar work but very much larger was tried for a short time and seems to work equally as well as the mica cage. This cage is about a foot square and fifteen inches in height. It consists of a wooden framework with fifty mesh wire gauze on one side and a sliding glass door on the opposite side. The other two sides, the top and bottom are covered with cheese cloth. A cross piece divides the bottom of the cage equally. One half of the bottom is immovable, while into the other half a door is fitted which opens downward. A small hole is bored in the center where the edge of the door and the crosspiece meet. Into this the stem of the plant fits. Another small hole is bored in any available space in the frame work to admit the

parasites and is closed with a cork stopper. Guy strings may conveniently be attached to prevent movement by the wind.

With this cage a larger portion of the plant can be enclosed, more squares can be supplied and more parasites placed under observation. The very size, however, prevents as close observations as are possible in the smaller cage. The oviposition of *Sigalphus curculionis* has already been observed in this cage.

Field Cages

To facilitate the increase of parasites in the cotton field, cages of fourteen mesh wire are used, in which are placed the hanging or fallen infested squares. Wire of this mesh enables all of the parasites to escape, but only a very small percentage of the weevils.

This cage is fifteen inches square and three feet high. It is covered on all sides, top and bottom with fourteen mesh wire. Five wire shelves are built in this cage, five inches apart, and on these are placed the infested forms. These wire shelves allow a better circulation of air, which keeps the material drier and also prevents heating and molding. One entire side forms the door, giving access to all of the shelves. When in the field this cage is kept free from infestation by ants or mites by placing the legs in a zinc tray containing two or three inches of water.

Other types of cages have been made but as yet their usefulness has not been proven by actual test.

President Forbes: Any questions to ask or discussion of this paper?

A Member: Mr. President, we had an interesting experience in trying to get parasites into tubes while studying a certain moth. We put them in a box fitted with glass tubes and we secured neither moths nor parasites. They had formed the habit of breeding in the dark and they would not come out to the light.

President Forbes: If there are no further remarks the next paper will be read by Mr. Parrott.

TREE CRICKETS AND INJURY TO APPLE WOOD

By P. J. Parrott, *Geneva, N. Y.*

During the past two years our attention has been directed to discolored areas on apple limbs, which have the appearance of being the early stages of a canker. These diseased spots have usually a dark

1. Dipterous breeding cage; 2. Outdoor parasite release cage; 3. Indoor breeding box; 4. Section of outdoor breeding house.

1. Large plant cage in operation; 2. Mica plant cage in operation; 3. Large plant cage and mica cages in operation.

reddish brown color, with a purplish tinge, and they are more or less circular in outline, varying from one quarter of an inch to one inch in diameter. From these there are occasionally lateral extensions of dead and depressed bark, which may be larger than the original wound. In comparison with published descriptions of the various canker diseases known to attack apple trees they resemble closely the pit cankers, which have been shown by Whetzel[1] to be due to the same bacterium which causes the blight of the pear. Our interest in these cankers was aroused by discovering that in many of these affected areas, usually about the center, there was a puncture in the bark, which generally led to an orthopterous egg. From these eggs young tree crickets have been hatched, which on attaining maturity were kindly identified for us by Dr. W. S. Blatchley as *Oecanthus niveus* De Geer.

Injuries of this character by this cricket have been mentioned by other writers. In 1898 Dr. A. D. Hopkins[2] described a cankerous condition of bark, which was common in the older apple orchards, and obtained eggs from apple branches, which he identified as belonging to a species of tree cricket. The diseased areas were attributed to blight and woolly aphis, which apparently became established in the wounds made by tree crickets in ovipositing. More recently Prof. C. O. Houghton[3] mentioned the occurrence of an insect's punctures, sometimes accompanied with depressed areas of bark, which were abundant on the trunks of young plum trees. In the wounds were eggs, from which specimens of *Oecanthus niveus* De Geer were reared. Similar punctures, although fewer in numbers, were observed in the bark of small apple and peach trees, and in raspberry canes.

In our studies on the habits of the Snowy Tree Cricket on apple, we have found that eggs of this species are quite common on neglected trees by the sides of ravines and highways, or on trees in orchards that are given indifferent tillage or are grown in grass, in which weeds to a more or less extent abound. Punctures in the bark by this species are quite common with both old and young trees growing under these conditions about Geneva. One Red Astrachan, surrounded by trees of the same age of other varieties, seems to be especially attractive to these insects. No difficulty is experienced in obtaining goodly numbers of eggs from limbs of this tree averaging about six inches in diameter down to branches as small as one inch in diameter. The bark

[1]Bull. 236, N. Y. Cornell Sta.

[2]Bull. 50, W. Va. Exp. Sta., p. 39.

[3]Fifteenth Ann. Rept. Del. Exp. Sta., p. 150, and Entomological News, Vol. XV, p. 57.

of the wood of these dimensions is very much punctured by crickets. and encircling many of these wounds are the discolored areas of bark as described.

This marked difference in the manner of oviposition by the Snowy Tree Cricket, from the common description of the habits of this species in this particular, prompted some experiments to obtain more data on the egg-laying habits of this insect on apple and raspberry. Fifty-five specimens of *niveus* reared from eggs deposited in apple bark, and thirty adults collected from raspberries were confined in various lots without mixing in breeding cages, in which were growing either nursery apples or raspberries. The insects oviposited freely, and more eggs were laid in the apples than in raspberries. In every instance only one egg was inserted in each wound, and the eggs were deposited irregularly and not in linear series, as with *O. nigricornis* Walk. In similar experiments with the latter species (*nigricornis*) the eggs were inserted in continuous rows, although occasionally single eggs were deposited, the female apparently having been disturbed before a larger number were laid. On young apples of four years of age the eggs of *niveus* were most abundant about the crotches of the trunks and larger limbs, while the eggs of *nigricornis* were deposited in the tips of the new growth.

The eggs of *niveus* obtained in the fall are generally pale yellow in color. The chorion has a delicate sculpturing, which is composed of fine lines, which are intersected, forming patches of tiny rectangles. The egg cap is yellowish or light brown, concealed with more or less pollinose. With magnification it has a honeycombed appearance and from the surface project cylindrical spicules, which are gently rounded at the tips. The measurements of twenty-five eggs gave an average length of 2,871 microns, and an average width at the broadest portion of 645 microns. The average length of the egg caps in this series was 511 microns and the average width was 521 microns. In the thick bark on the larger limbs and about the base of fruit spurs the eggs are inserted at an angle of 30° to 45°, while in the thin bark of the younger growth they lie almost flat on the wood. The tissues surrounding them become hardened and form a tough, protective case, from which it is difficult to remove the eggs. In the breeding cages oviposition commenced on August 20 and continued until August 31. Hatching of eggs began on May 25 and lasted until June 18. The first adult was observed on July 20.

The eggs of *nigricornis* vary from a light to a medium chrome yellow. The egg caps are cream or a light yellow, with sometimes a tinge of brown on the ends. Examined with a microscope they have a

honeycombed appearance and short spicules. The chorion has a sculpturing that is similar to *niveus*. The average length of twenty-five eggs that were measured was 2,924 microns and the average width on the broadest portions was 580 microns. The length of the egg caps averaged about 342 microns and the width 447 microns. The eggs of this species differ from those of *niveus* in that they are more slender and are of a deeper yellow in color, and have shorter spicules on the egg caps. Injuries to raspberry canes by *nigricornis* are common.

President Forbes: Discussion of this paper is now in order.

Mr. Hopkins: Mr. President, I am especially interested in this paper and am glad to know the species concerned, because it has always been a mystery to me as to what species caused the damage. My observations, however, are that the eggs were deposited almost invariably in pairs and that in a great many cases the canker did not follow the puncture. (Mr. Hopkins then exhibited a plate which he had published in Bulletin 50, West Virginia Agricultural Experiment Station, illustrating the work of tree crickets.)

Mr. Parrott: I do not want to leave the impression that the cankerous condition was common with all the punctures. This was not the case. It was present, however, with many of the punctures.

Mr. Slingerland: We have one of our graduate students at Cornell working on this problem and his results are similar to those of Professor Parrott.

President Forbes: Any further discussion? The next paper on the program will be read by Mr. H. E. Summers.

THE DISTRIBUTION OF SAN JOSE SCALE IN IOWA

By H. E. Summers, *Ames, Iowa*

As the line marking the northwestern limit of injury by the San José scale in the Mississippi Valley passes thru Iowa, the localities in which it has establisht itself in the state seem worthy of record. It has so far been found doing injury only in five counties, namely: Decatur, Lee, Louisa, Mahaska and Linn. Only one center of distribution has been found in each of these localities. Four of these counties are south of the 42d parallel; only Linn County being north of it. Four of the outbreaks were in orchards, in all of which some trees were killed. The fifth case was in a nursery in which an isolated block of transplanted trees had been standing for several years, and were found generally infested. Some trees were dying. This block

had been inspected two years before, but no scale had been found. altho the infection must have then existed, as no trees were after that set in the block and the possibility of infection from surrounding territory seemed to be excluded.

In all the above cases, the scale when discovered was in a thriving condition and it was evident that it was only a question of time, unless treatment were given, when the trees would be entirely destroyed. The Decatur County infection was discovered in the early spring of 1901. It was sprayed with whale oil soap in April of that year and with sulfur-lime mixture in the spring of 1902. Complete eradication of the scale, as was proven by annual inspection for four years afterwards, was the result. The Lee County infection was discovered in May, 1906. It was kept fairly well in check during that summer by two sprayings, with a third upon some trees, with kerosene emulsion. It was sprayed in the spring of 1907 with sulfur-lime mixture. The work was done by the owner and, as inspection soon after showed, the trees had not been entirely covered. By autumn many trees were again badly infested. In the spring of 1908 this orchard was again, and this time carefully, sprayed with lime-sulfur. Inspection this autumn (1908) shows that the treatment was not effective in exterminating, altho the number found on any tree that had been sprayed was very small. One peach tree quite distant from the orchard sprayed, and the only tree on the place that had not been treated, was found this fall to be thickly covered with the scale. This tree had been inspected in the spring and no scale found upon it. It seems evident that the sulfur-lime remained upon the sprayed trees in sufficient amounts during a considerable part of the summer to prevent the establishment and rapid multiplication of the scale. The other two cases of orchard infection have been discovered during the present autumn and no treatment has yet been applied. In the case of the nursery, everything found infected and near the infected stock was cut out and burned; and as this nursery had for two years been establishing itself on new grounds several miles away, it is hoped that complete eradication has been accomplished.

Perhaps greater interest attaches to the question as to how certain Iowa grown nursery trees on which the scale has been found became infested. Several shipments from Iowa have been found by inspectors in other states infested with San José scale. In all but two cases. these trees have been traced back and found to have been shipped into Iowa from nurseries in other states. In two cases, however, the trees were grown in large nurseries in the southwestern part of Iowa far removed from any known infestation. In one case a few scattered

scales were found on each of three trees out of a lot of nine hundred shipped to Maryland. The other trees were hand inspected and were apparently clean. Six hundred trees, all of the remainder grown in the block, were subsequently hand inspected by me personally at the nursery in Iowa and no scale discovered. In the other case a single tree was found infected in a large shipment made to New York. The question of the source of this scale is a puzzling one. It seems incredible that it should have been introduced on the scions and not have multiplied more in the three years, during which all the infested trees had been growing in the nursery. On the other hand the orchards for at least a mile in every direction from the nursery have been examined with sufficient care to have insured the discovery of any severe infection.

To summarize, the San José scale has twice been found on Iowa grown trees sent out from the nurseries. Once, in a nursery from which it is improbable any infested trees have been sent out, and in four localities, all in the eastern and southern part of the state, in orchards. In three of these it still exists.

Mr. Washburn: I shall have to criticize Mr. Summers' statement that Iowa represents the most northern spread of the scale in the Mississippi Valley, because it has been known around Madison, Wisconsin, for two or three years, and in South Dakota I heard this winter that the scale had lived through two winters. The stock was burned up the following summer, so we do not know whether it would have gone through another winter or not. It is rather strange, but we have not found it in Minnesota yet. I have no doubt it is pushing its way north, and we have inoculated, in a muslin cage, out of doors, fruit trees with scales that we have had sent to us, and left the top of the cage open during the winter, and have had scales live through last winter. I don't think we ought to regard Iowa as the most northern limit in the Mississippi Valley. I would like to ask Mr. Summers if he has found it around Charles City.

Mr. Summers: We have never found it anywhere except at the places mentioned in my paper.

President Forbes: If there is no further discussion, the next paper will be presented.

Mr. Quaintance gave a brief verbal statement of the facts contained in the paper, which follows:

THE SELF-BOILED LIME-SULFUR MIXTURE AS A SUMMER TREATMENT FOR THE SAN JOSE SCALE

By A. L. Quaintance, *Washington, D. C.*

A self-boiled lime-sulfur wash, that is, one in which the cooking is done entirely by the heat generated by the slaking of the lime, has been for some years more or less used as a substitute for the well-boiled wash for dormant tree treatments for the San José scale. As obviating the necessity for a cooking plant, this self-boiled wash, if effective, would have much to commend it, but unfortunately, in most cases, it has not given satisfactory results in controlling the insect and is, perhaps, now but little used. Analyses of such washes have shown that, as ordinarily made, only a small part of the sulfur present passes into solution, though the free sulfur present is left in a very finely divided condition. The amount of heat generated by the slaking of the lime will vary considerably, depending upon its purity and as to how the mixture is handled, as the use of hot or cold water in slaking, the length of time the mixture is allowed to stand after slaking, whether the vessel be covered, etc. Analyses of washes made in a way to generate and conserve the maximum amount of heat, show that a relatively high percentage of sulfur may be rendered soluble, approximating in fact the amount obtained in a well-cooked wash. Variations in method of preparation, therefore, could well account for the different results reported by orchardists and experimenters in the use of self-boiled washes. The self-boiled caustic-soda wash, quite a different preparation, has been used more successfully as a dormant-tree spray for the San José scale, as the heat from the lime is supplemented by that resulting from the caustic, and the chemical reaction is, furthermore, quite different.

The supposed causticity of self-boiled washes has had the effect of excluding them from among possible sprays for use on trees in foliage and, although these have been much experimented with by entomologists and others, no one apparently has tried a self-boiled wash as a summer treatment for the San José and other scales. The recent investigations of Prof. W. M. Scott, of the Bureau of Plant Industry, of self-boiled lime-sulfur mixtures as fungicides for use on the peach in the control of brown rot and scab, and on apple in the control of scab, bitter rot, etc., have shown, contrary to the general impression, that self-boiled mixtures may be prepared in a way to permit of their use with perfect safety on trees in foliage. This is a very important discovery from the standpoint of plant pathologists, as furnishing a

new fungicide, especially adapted to use on stone fruits, and one of the features of the work, as was pointed out by Mr. Scott, was the probable usefulness of this mixture as a summer spray for the San José scale.

Unquestionably, the destruction of scale insects may be best accomplished by applications to trees when in a dormant condition, as at this time a very strong wash may be used and there are no leaves to interfere with thorough applications. It often happens, however, that for one reason or another the winter treatments have been neglected or imperfectly accomplished, and the life of the trees will be greatly endangered if the scale is allowed to develop unchecked through a season. Also, the presence of the scale in orchards is often first discovered after the trees have put out foliage and the owner desires to promptly begin remedial work, not waiting until the dormant period of the trees, several months later. For these reasons treatments in summer are often very necessary. Recourse has usually been had to dilute kerosene emulsion, whale-oil soap or other solutions, which, while effectively destroying any crawling lice present at the moment on the trees, have but little effect upon the older individuals or young, which are somewhat protected by the scale covering. The treatment is, therefore, but a temporary check to the insects, and to be of much value in lessening the insects, must be often repeated.

The past summer some tests made under the writer's direction of a self-boiled wash as a treatment for the San José scale seem to indicate that we have in this a valuable addition to our list of scalecides for use on trees in foliage.[1] The experiments were made in two localities, namely, in Maryland, in the vicinity of Washington, by Mr. P. R. Jones, and near Saugatuck, Michigan, by Mr. R. W. Braucher. In the course of some spraying experiments with self-boiled washes for the brown rot of peaches made at Bentonville, Arkansas, in a badly scale-infested orchard, Mr. J. B. Rorer, of the Bureau of Plant Industry, was also able to make some observations on the effect of these treatments upon the San José scale.

The tests made by Mr. Jones on peach at Marshall Hall, Md., while on a small scale, are believed to be quite reliable. Two plats were sprayed just as the young lice were beginning to crawl, and the second plat received an additional treatment about four weeks later. One plat was left unsprayed for comparison. Examinations of the sprayed trees shortly after the first treatment had been made showed

[1]The formula used was that given in Circular No. 1, Bureau of Plant Industry, U. S. Dept. Agriculture, by W. M. Scott.

that there was but little immediate effect on the older scales, though the crawling young and recently set individuals had been killed. Later examinations during the season revealed that, although the adults were alive and actively breeding, but few of the young lice established themselves, owing, no doubt, to the presence of the wash upon the trees. This result is quite in harmony with that well known to result from the use of the well-boiled wash applied in spring shortly before the buds appear; that is, there is a continued effect which is perhaps more important than the direct insecticidal action of the wash. At the final examinations of these plats in the late fall, the condition of trees sprayed once was notably better in regard to freedom from scale than trees not sprayed, and on the plat sprayed twice the scales had been very largely cleared from the trees, approximating in fact a condition resulting from a very thorough use of the well-boiled wash during the dormant period.

In the case of the peach orchard at Bentonville, Arkansas, reported upon by Mr. J. B. Rorer, these trees were very badly infested with the San José scale, many of them being almost completely incrusted. The first application of the wash was made on May 5th, using the formula: lime 15 pounds, sulfur 10 pounds, water 50 gallons; and this formula was again applied on May 21st to supplement the earlier treatment, since this was very imperfectly applied. Another treatment was given June 20th, using the 10–10–50 formula and a final treatment July 9th, using the same strength of wash. This orchard had had no previous attention whatever in regard to controlling the scale, but the treatments very largely cleared the insects from the trees. Many young lice, which continued to develop from the breeding insects present failed to establish themselves, and thus, as the older insects died, the scale gradually disappeared. The unsprayed trees at the close of the season were in a very serious condition. Many of the larger limbs and twigs had been killed and all of the trees greatly enfeebled.

These two instances of the practical cleaning of peach trees of scale, in view of the serious infestation which existed, are considered very favorable to the usefulness of the wash as a summer spray, especially since it will doubtless come into extended use in the control of fungous diseases, as already mentioned. In the case of peach, it seems probable that when used as a fungicide for peach scab and brown rot, it will at the same time sufficiently destroy the scale as to obviate the necessity of the usual dormant-tree treatment. Great thoroughness in applications, however, would be necessary, reaching the body, limbs and twigs.

In the tests in Michigan, carried out on apple by Mr. Braucher, it was planned to determine to what extent summer applications of the wash would prevent the young lice from settling upon the fruit when used supplementary to the usual dormant treatment, and when used as a summer spray only, as might occur in its use as a fungicide. Several things interfered to prevent the execution of the work as planned and the results were variable. The importance of the dormant treatment, however, was clearly shown, and also of a summer treatment about as the young scales were due to appear from adults which escaped destruction, for protecting the fruit from spotting.

The principal reason for presenting this brief note is to call the attention of entomologists to the possibilities of using the self-boiled lime-sulfur mixture as a summer spray for the San José scale, so that if desirable it may be tested under a wider range of conditions.

The question naturally arises as to the effect of the addition to the wash of arsenicals, such as arsenate of lead, or Paris green, necessary especially in the case of the apple. When the former is added to the wash important evident chemical changes result, the wash taking on a dark gray or blackish color. Concerning these reactions, Mr. J. K. Haywood of the Bureau of Chemistry, to whom we submitted the question, says: "First, in regard to mixing lead arsenate with lime-sulfur: the lead arsenate is decomposed to a certain extent, lead sulphide and calcium arsenate being formed. The latter is somewhat soluble, unless an excess of lime is present, in which case it is rendered insoluble. The lead sulphide formed being insoluble, would remove some of the sulfur from the solution, but this amount would be relatively small and would probably not materially lessen its efficiency.

"In the case of Paris green and lime-sulfur, the former appears to be entirely broken up, some of the arsenic going into solution, as arsenic sulphide. A part of the copper also goes into solution, the remainder being precipitated as sulphide. The presence of an excess of lime in this case does not entirely render the arsenic insoluble. The latter practice, therefore, would seem to be of doubtful expediency."

PRESIDENT FORBES: This paper is now open for discussion.

MR. PARROTT: I am very glad to hear the results secured by Mr. Quaintance, because we have had similar results in our experiments with this mixture. We have also found that it is very effective in controlling the brown rot on sweet cherries. A number of fruit growers in New York have tried this spray and, while it did not cause any

injury in our experiments, there is one orchardist who states that it injured the foliage of peach trees. I would like to ask if this wash is uniformly safe.

Mr. Quaintance: I would say that it differs a good deal, according to the manner in which it is prepared. I see Mr. Scott in the audience, and I would like to ask him to state his experience in preparing it.

Mr. Scott: In the experiments that we made in 1907, this wash caused no injury to peach foliage and we were greatly surprised at the results. They were published with some "fear and trembling" in Circular No. 1 of the Bureau of Plant Industry. When we began work again last spring we made a foliage test, using mixtures made in various ways, and found that by using boiling water and allowing the hot mass to stand for thirty or forty minutes, the cooking continued and a large per cent of the sulfur combined with the lime. By slacking the lime with cold water and, as soon as the violent boiling ceased, adding more cold water to reduce the temperature and thus prevent further chemical action, we secured a mixture which was not injurious to peaches or apples. The point is to stop the cooking process immediately after the lime is thoroughly slacked.

As to the strength, we used 10 pounds of lime and 10 pounds of sulfur to fifty gallons of water and secured good results against the peach scab and fairly good results against brown rot. We found that by diluting the mixture down to 6–6–50, the results were not quite as good.

Mr. Gossard: If it is not getting too far away from the subject. I would like to ask whether there has been any experiments tried in combining arsenate of lead with the self-boiled lime-sulfur wash? One or two fruit growers wrote me that they had had a rather disastrous experience in this connection. I made some experiments on apple, but used the two washes separately. This treatment controlled scab about as well as Bordeaux mixture, but it did not entirely control apple scab in Ohio this year. I would like to know if these two insecticides can be safely mixed.

Mr. Braucher: I had a similar experience in my work in Michigan. After the trees had been sprayed with Bordeaux mixture and then sprayed with the self-boiled lime-sulfur wash there was a decided change. The leaves became a russet brown and looked as though they were very badly injured, but later observations showed that there was no special injury to the foliage. I tested that point in the laboratory by taking some lime-sulfur mixture and adding arsenate of lead to it; by adding sufficient arsenate of lead I took

every bit of color out of the lime-sulfur mixture and a slate colored precipitate was thrown down, showing that there is a pronounced chemical reaction. I do not consider it a safe mixture to use.

Mr. Quaintance: Mr. Haywood of the Bureau of Chemistry made an analysis for us and found that the arsenate of lead, when added to the self-boiled wash was changed. Lead sulfide is formed and arsenate of lime. The arsenical is present in the form of arsenate of lime and, of course, might do considerable damage, as there might be more or less uncombined arsenic. Paris green has also been used in combination with the wash. Mr. Scott had some tests of these combined sprays under way in the Ozarks, and I saw the plats myself, and we were not able to detect the slightest injury.

President Forbes: The next paper will be presented by Mr. Sanderson.

NOTES ON RECENT EXPERIMENTS FOR THE CONTROL OF THE CODLING MOTH

By E. Dwight Sanderson, *Durham, N. H.*

Investigations of the life history of the Codling Moth in 1908 add little that is essentially new to the results given this association last year.[1]

Experiments concerning the time and method of spraying have, however, more fully confirmed our previous work and we feel we have established certain points which need emphasis. The results of three years' work also show the necessity for the most careful arrangement of plots and recording of data in order to secure definite results. The details of our results will not be given here, but can be studied in the forthcoming 20th Report of the New Hampshire Agricultural Experiment Station.

1. **Time of Spraying.**—All experiments have shown the first spraying just after the petals fall to be the most efficient, giving an average benefit of 82% when used alone.

Formerly we were advised to spray a week or two after this. Under some climatic conditions this may be advisable, but there is little reason for an application for the Codling Moth in New England at that time. The second spraying should be applied when the eggs are hatching, three or four weeks after the first spraying. An average of five plots given this spraying only show 72% benefit for the season. Recently there has been a tendency to magnify the value of thorough

[1] Journal of Economic Entomology. I, 129–140.

work with the first spraying and to depreciate the value of later spraying if the first was properly made. In reviewing the records of previous experiments the writer has been unable to find any satisfactory experiments to show the value of a single spraying when the eggs are hatching. Our work has been carried on on so large a scale, however, that there seems to be no doubt of the value of a single-spraying at this time. Observations of practical men confirm this. Mr. H. L. Frost advises me that he has frequently observed orchards sprayed for gypsy moth in late June and early July, which showed practically no wormy fruit as a result of this one spraying. The fruit grower should not be encouraged to neglect the more important spraying when the petals drop, but he should know that if for any reason he is unable to spray then that a thorough spraying three or four weeks later will save nearly three fourths of his loss from worminess.

Several experiments have previously indicated this, but the data was inconclusive. Thus Cordley in 1902[2] showed that where the check tree had 20% wormy, trees given the "2d, 3d and 4th" sprayings, the first being omitted, had but 5% wormy, while those which had the first spraying also had but 3% wormy. Green and Houser in 1905[3] show that in one orchard where 56% of the picked fruit was wormy on unsprayed trees but 11% was wormy where spraying was given only in July and August, against 5% wormy where six sprayings were given in June, July and August. In another orchard, where 52% of the checks were wormy when picked, but 5½% were wormy in trees sprayed June 22d and July 16th, while those given two previous sprayings had only 3% wormy. These results, however, are conflicting and the methods employed were hardly accurate enough to be conclusive.

Recently Gossard[4] found that where unsprayed trees had 46% wormy, spraying when eggs were hatching gave practically 12% wormy, while the first spraying alone gave 6½% wormy. Observation has shown that the eggs are laid on all parts of the foliage, that the newly hatched larvæ feed on the foliage, and that many enter the calyx by eating directly through the calyx lobes, upon the outside of which a large amount of poison is always lodged. It is evident, therefore, both from our knowledge of the habits of the insect and from the direct results of experiments, that spraying when the eggs are hatching so as to cover the foliage and fruit is of great value and should not be neglected.

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[2]Bulletin 69. Ore. Agr. Exp. Sta., p. 150.
[3]Bulletin 160. Ohio Agr. Exp. Sta., pp. 205–208.
[4]Bulletin 191. Ohio Agr. Exp. Sta., p. 116.

2. **Method of Spraying.**—Further experiments have but confirmed our previous assertion that in New England a drenching spray, as advised by some western entomologists, is of no value as far as driving the spray into the calyx is concerned. The reason is not far to seek if the apples are studied. Professor Slingerland[5] has recently shown that the calyces of apples may be closed before the stamen bars are sufficiently shrivelled to allow the spray to penetrate to the lower calyx cavity. Many careful examinations of Baldwin apples have shown this to be absolutely true of that variety under our climate. All of our experiments have been made on the Baldwin, as it is practically the only commercial variety in Northern New England. Our heavy spraying was done this year with Bordeaux nozzles at an angle, at 100 to 120 lbs. pressure, with a gas sprayer, and yet we were unable to find the slightest of spray in the lower calyx cup.

Where the calyx lobes remain open long enough to permit spraying between them after the stamen bars commence to shrivel, there can be no doubt that it is the desirable time to spray and that the spray should be shot into the lower calyx cavity, but that such a spraying is so all-efficient as recently claimed seems hardly to be demonstrated by the evidence submitted. There are also striking differences in the methods used to reach the lower calyx cavity by the chief advocates of its use. In his first publication[6] Ball tells us that a barrel pump giving 85 pounds pressure enabled him to place the poison in the lower calyx, and later[7] he states that a pressure of 85 to 120 pounds is correct. But Melander[8] insists that 150 to 200 pounds pressure is necessary and scouts the idea that satisfactory work can be done with 85 pounds pressure, referring to the work of Doyd in Illinois.[9]

The value of driving the spray into the lower calyx cup was first suggested by Ball in 1904. In Bulletin 95 of the Utah Station he describes the method used and in the summary states:

"To get the best results from early sprays they must be applied in the form of fine drops driven with force straight into the bottom of the calyx cups." No experiments are recorded, however, in which this method is compared with an ordinary mist spray thoroughly applied. Nor has he later described any experiments to prove the superior value of such a method of spraying. The value of such spraying seems to be based on the belief that most of the young larvæ are

[5]Journal of Economic Entomology. 1:352.
[6]Bulletin 95. Utah Agr. Exp. Sta.
[7]Bulletin 67. Bureau of Entomology, p. 63.
[8]Popular Bulletins, 5. Wash. Agr. Exp. Sta., p. 5.
[9]Bulletin 114. Ill. Agr. Exp. Sta.

killed by eating the poison in the lower calyx cup, because dead larvæ were frequently found in the lower cavity and never in the upper cavity.[10]

Is there any reason why a larva may not have eaten the poison in the upper calyx cavity or when eating through a sepal into the lower calyx cavity, and then died in the lower cavity? Because poison is placed in the lower cavity and dead worms are found there is but circumstantial evidence that the larvæ are killed there. If so, how do we secure equally good results in New England, where it is impossible to spray into the lower calyx cavity? In his later paper Ball describes experiments made in 1905 (*l. c.*, p. 57, 59) in which plots were sprayed with the first, second and third spraying separately and in combination. "The first spraying was applied just after the blossoms fell, the second ten days later, and the third fifteen days after that." With the second spraying only 76% of the first brood of larvæ were killed and 31% of the second brood; and with the first spraying only 83% of the first brood and 39% of the second brood. In other words the second spraying alone was about 7% less efficient than the first. Later (*l. c.*, p. 70) he states that about ten days after the blossoms fall the stamens have shrivelled and that then is the best time to spray. (See *l. c.*, fig. 4.) The plots given the second spraying only were sprayed just ten days after the blossoms fell and, therefore, just at the best time, but they show 7% less benefit than those sprayed just after the blossoms fell. This difference is even more striking in his results in 1906 (*l. c.*, p. 66), when the second spray gave but 64% killed in the first brood and 38% in the second brood, while the first spraying alone gave 84% and 57%. Strange enough, however, the first and second together were poorer than the first alone, killing but 72% of the first and 57% of the second broods. There may have been some circumstances concerning these experiments not described by the writer, but the data submitted hardly seems to demonstrate the necessity of reaching the lower calyx cavity.

More recently Melander[11] has made the statement: "We have shown that the first spraying *can* be so thoroughly applied that other sprayings are hardly necessary." Just how this was "shown" we are unable to determine, as every plot described in the bulletin had from two to four sprayings. Nor do we find described any direct comparisons to show the difference in the same orchard between trees sprayed with the "old-fashioned" and "up-to-date" methods. On the other

[10]Bulletin 67, Bureau of Entomology, p. 73.

[11]Bulletin 86. Wash. Agr. Exp. Sta., p. 15.

hand, the unsprayed trees in one orchard (*l. c.*, p. 15) had 74% and 95% good apples. Surely, it does not take extraordinary spraying to secure 100% perfect fruit where 95% are perfect on unsprayed trees. In the orchard showing the most perfect fruit four sprayings were given, but there were no check trees left unsprayed. So far as we can ascertain, this is the evidence by which the supreme efficacy of the new methods is "shown." In 1907, however, Melander and Jerme[12] give the results of spraying sixty-three trees with various brands of arsenate of lead in an orchard in which the unsprayed trees had but 42% good fruit. The trees sprayed four times had an average of 96.5% good fruit. A power sprayer and Bordeaux nozzles were used in 1907, but nothing is said about the efficacy of thorough work at the time of the first spraying. However, 96.5% good fruit were secured against 98% in 1908, and the 1907 records are certainly much more accurately given. Furthermore, in one plot in 1907 the fourth spraying was omitted and gave but 84% good fruit, though it had the first three sprayings. Concerning this Melander and Jerme there remark: "The greatly increased percentage of worms in these trees clearly shows the necessity for this last spraying, which must be given for success," which conclusion is based on much better evidence than that offered in 1908, to show that it is *not* necessary. Also in Bulletin 77 of the Washington Station (p. 64) Melander and Jerme have shown that fairly good results were secured when the first spraying was omitted in an orchard where 56% was wormy on unsprayed trees.

The writer does not wish to be understood as disbelieving in the value of driving the first spray into the lower calyx cavity, where varieties and climate make such a method possible, but he does wish to see some experimental evidence which will fairly demonstrate the desirability of such spraying, and particularly to support the claim that one thorough spraying with but one pound of arsenate of lead per barrel is sufficient, if rightly applied, when the evidence submitted goes to show the great value or necessity of also spraying the foliage and apples, in addition to the spray in the calyx in order to satisfactorily control the Codling Moth.

3. **Method of Experimentation.**—Several years ago[13] the writer showed the absolute necessity of treating several trees alike in order to get an average upon which a comparison of different treatments might be made, on account of the variation in individual trees treated alike. Yet experimenters have gone on counting but one or two trees

[12] Bulletin 81. Wash. Agr. Exp. Sta., p. 6.

[13] Delaware Agr. Exp. Sta. 13th Rept., Table VII.

and ask us to give credence to their conclusions. Our recent experiments have even more strikingly shown this, and in his report the writer has taken space to give the complete records of all the plots sprayed the last three years. The variation between trees side by side in the center of large plots is so considerable that the utter futility of experiments with but one or two trees in a plot is at once apparent. This is strikingly shown in the work of Lloyd (*l. c.*, 388, 390), in which for two years eleven trees were treated in as many ways and the percentage of wormy fruit was greater where two heavy applications were given than but one application in both years.

Our records also show the necessity of comparing results in percentages rather than by the number of wormy apples, unless the trees are all of uniform size, evenly fruited, regularly placed, and bearing the same year.

The arrangement of plots must also receive more careful consideration. Unsprayed check trees must be isolated and yet of sufficient number to represent the orchard. The plots must be large enough so that the central trees in each will not be materially influenced by the next plot. In our own experiments we have endeavored to determine the proportion of larvæ killed in the calyx by eating the side of the apple and by eating the foliage, and we have secured considerable evidence on these points. But we have been forced to the conclusions that the orchards available are too small and are not uniform enough to secure comparable results. Our records will be found to be filled with inexplicable inconsistencies and contradictions, which can be due to nothing else than the lack of uniformity in the trees, as regards the year bearing, number of fruits borne and position. To determine such points or the influence of any given spray on the first and second brood, a large orchard is needed, all the trees of the same variety, or of but few similar varieties, with plots of each variety treated similarly, so that their similarity may be determined by the comparison of those treated the same. The trees should all bear uniformly as to number of fruit and the year bearing. The plots should contain 21, or preferably 35, trees and the fruit from five central trees should be counted, both windfalls and picked fruit. As orchards offering such conditions are not available in New Hampshire, we have been compelled to forego further investigation of these problems.

If we are to secure any results upon problems of this kind which are at all conclusive and which will not be as readily contradicted, we must arrange our experiments on a larger scale and with greater exactions and care. The conditions surrounding such biological problems are so varied that too great care cannot be taken in arranging

different plots which are to be compared. Experiments in the control of the Codling Moth have now been made for a quarter century, at a large cost, yet many of the most elementary matters concerning the effect of such treatment are still unsolved. Unless more careful and elaborate methods of experimentation are adopted, they will remain befogged by the mass of data based on single trees and "sample counts," while time and money are being wasted without adding to our knowledge.

At the present time, when increased emphasis is being placed upon agricultural research of a high order, we must see to it that the methods we employ are of as undoubted scientific accuracy and thoroughness as possible. Otherwise we need not complain if funds for investigation are given to investigators in other sciences in which better methods have been developed, whereby more accurate and incontestable results can be secured. Let us give more attention to our methods of work and let us remain agnostic until we have secured such a mass of conclusive evidence as will enable us to definitely establish the facts. By so doing we will avoid burdening each other and the public with opinions, so many of which have in the past been quickly refuted, and which tend to lessen confidence not only in the individual investigator, but in the science he represents.

President Forbes: Discussion of this paper is now in order.

Mr. Taylor: Mr. President, I am more or less familiar with the methods of Codling Moth spraying in the West, and also in the East, having had some experience in fighting this insect in Colorado and in the Ozark district in Missouri, and I must say that the difference of opinion that has sprung up between eastern and western methods is somewhat exaggerated. After all, I don't think there is such a difference in conditions between the West and the East, and I believe that the principal difference in opinion is not so much a difference in the entomological value of the sprays as it is a matter of expediency in applying them. For instance, the people of the West who are equipped with power sprayers contend that the spraying with power outfits at a high pressure is much better than spraying with barrel pumps, with pressures of 100 or 125 pounds. In an experiment that I conducted in the Grand Valley in western Colorado with a power outfit we sprayed a row of trees at 100 pounds pressure and on the next row we used 180 or 200 pounds. We found that we sprayed more trees with the high pressure and that we sprayed out our tank in a much shorter time. That is the point that the orchardist is after. He wants to get his work done quickly and spray as many trees as possible. I think that

all of you who have had experience with power sprayers will agree with me in this respect.

As to the value of early sprays, Mr. Sanderson has mentioned controlling the Codling Moth by omitting the first spray. I am certainly of the opinion that the western people have the right idea in placing great value and importance on the first spray, the one given immediately following the dropping of the petals. This year I applied the methods followed in the West in an apple orchard of about 1,500 trees in the Ozarks. I applied three sprays, the first of which I gave immediately following the dropping of the petals, but before the calyx lobes had closed, in a drenching spray. I applied two lighter sprays, using a fine mist, which was used so as to leave a maximum amount of the material coated over the apples. I do not believe, from my experience, that a single spraying will control the Codling Moth in apple orchards where the infestation is severe. I do not think that is possible, from the fact that according to counts made by me while in the western Colorado section they showed that about two thirds of the worms entered the calyx end and about one third at the side of the apple, and any spray applied so as to fill the calyx only, whether in the upper calyx or the lower, would not destroy the 33% that might enter the side of the apple. I think that the better orchardists in the West are coming to this plan of applying the first spray with a heavy drenching spray after the dropping of the petals, and using later a fine mist spray. I produced this year, out of a count of some four thousand apples, only six infested with Codling Moth, and that was in Missouri, a result, I think, which will compare favorably with any shown in the Northwest. After all, if the entomologists of the West and Northwest could come to our meetings and talk this matter over, there would be fewer chances for such differences of opinion as seem to have arisen.

President Forbes: The next paper on the program is by Mr. E. D. Ball, Logan, Utah.

IS ARSENICAL SPRAYING KILLING OUR FRUIT TREES?

By E. D. Ball, *Logan, Utah*

In Bulletin 131 of the Colorado Experiment Station, Dr. Wm. P. Headden comes to some very startling conclusions with reference to the effect of continued arsenical spraying on the life of our orchards. As the truth or falsity of these conclusions is a matter of vital and immediate importance to the fruit growers of the West, if not to those

of the whole country, it is important that every fact bearing on the case should be made known at once.

Briefly summarized, the bulletin sets forth that the apple trees in the Grand Junction District of Colorado are affected with certain troubles, which Mr. Whipple in Colorado Bulletin 118 calls "root rot," and divides into two classes. One of these troubles Mr. Whipple dismisses with this statement: One form, which is proving the least destructive of the two, seems to show no preference for varieties, and confines itself to that part of the tree below the ground." The other condition described by Mr. Whipple at some length is apparently our old friend, long and widely known as "collar rot." He says, "It works exclusively on the Ben Davis and Gano," and describes the dead areas of bark, the girdling of the crown, the early ripening of the foliage and other characters well known as characteristic of this condition.

Doctor Headden, who had previously predicted that arsenical spraying would be dangerous, made a trip to this section late the next season and was shown a tree, one side of which had been killed by dumping the soluble arsenite of soda into a ditch, from which that side of the tree received its water. From this tree the doctor took samples of the root, trunk and branches and on analysis found that they contained arsenic. He afterwards collected samples of thirteen other trees that were dead or dying from the "root rot" conditions described above and on examination found arsenic present in each one of them. He also examined the soil under some of these trees and found arsenic present in considerable amounts but in an insoluble form. As the result of these tests the doctor comes to the following conclusions, which I quote:

"I regret that I can see no other conclusion than that the corroding of the crowns, the killing of the bark, the staining and final destruction of the woody fibers, the early dropping of the leaves, presaging the early death of the tree and its final death a few months later, are caused by arsenical poisoning."

Doctor Headden in this statement is talking about conditions which he states he observed "from near Fruita, almost to Palisade, and in the neighborhood of Delta," and which he believes already involves the principal apple-growing sections of Colorado and of which he says: "It is also true that literally hundreds of trees have already died or are sick."

If Doctor Headden is right in his conclusions as to the cause of the death of those "hundreds of trees," he has given us a warning, which, if heeded in time, will prevent a course being pursued by the fruit

growers of the West, which would have caused millions of dollars of loss in ruined orchards. If, on the other hand, he is mistaken in his conclusions, the publication is most unfortunate, as it will, no doubt, cause a decided reaction against a now highly successful method of spraying and bring consequent financial loss to the fruit industry.

It might be well to state in this connection that Doctor Headden is a well known and thoroughly reliable chemist, and there is no reason to doubt the absolute accuracy of any of his chemical findings. He is not, however, a horticulturist or a plant pathologist, and has taken little interest in orcharding, and his conclusion that the trees he found dying or dead in other orchards were affected in the same way as the ones known to have been killed by the soluble arsenic is open to serious question. He places a great deal of weight on the similarity in appearance of the bark and the discoloration of the heart wood, as illustrated by his figures, but as far as anyone can discover from the statements of the bulletin, he made no examination of the apparently healthly trees in these same orchards nor of dead trees in orchards that had not been treated with arsenical sprays. In fact, nowhere in the bulletin can we find a statement that would lead us to believe that he is aware of any cases of trees dying where no arsenical sprays have been used.

Alkaline Ground Water Killing Trees

In investigating orchard conditions in Colorado and Utah the writer has had frequent occasion to study both conditions described by Mr. Whipple. The condition first mentioned, in which there is no preference for varieties, has destroyed several hundred acres of orchards in Utah and western Colorado, and in every instance where this has been investigated it has been found that the ground water was very close to the surface, or at least came up during some part of the year, and contained a large percentage of alkali.

In one section, where the greatest loss has occurred, a survey of the region, showing the depth of the ground water, has been made and in every case the worst affected orchards are located in the region where the water is closest to the surface, as shown by this map. Over one half of the orchards in this section had never been sprayed and in others many trees died before they were large enough to bear fruit. In other cases trees are dying from alkali, where the ground water is not normally close to the surface but where at certain seasons of the year irrigation on the higher lands brings it up temporarily.

Where the ground water is constantly close to the surface young trees grow with great vigor until the roots reach this standing water, and then they gradually become sickly and yellow, the leaves ripen

prematurely and as the trouble progresses the bark becomes mottled and discolored with brown areas and the trees gradually die. In cases where the ground water fluctuates through irrigation, the tree may be growing in an apparently healthy and vigorous manner one day, by the next the leaves may begin to show brown upon the edges, and in a few days the entire foliage will look as if it had been sprayed with some soluble arsenical solution. If the ground water remains close to the surface for some time the bark will become mottled and the trees will die. If it soon falls they will often drop their burned leaves and, if not too late in the season, the young shoots will push out and form a partial green covering again. This occurrence is so common in some of the low lying districts adjacent to the Great Salt Lake, where it is impossible to drain, that it occasions little comment, everyone understanding the conditions that produce it.

On the Central Utah Experiment Station an orchard was planted just below an irrigated bench, or table land, and the seepage water from the side of this bench rendered the upper end of the orchard almost impassable, and the trees gradually died. A trench was dug to intercept this alkaline water, after which the ground below the trench dried up, the remaining trees recovered and the orchard was apparently saved. A year later a cave occurred in the trench and the water began to rise again. Within a few days the trees in this area were brown and scorched, as if swept by a fire. The trench was then cleaned and repaired, the water soon subsided in the orchard and all the trees except one put forth a new crop of leaves. No arsenical spray had ever been applied to this orchard until after the trees died.

Conditions similar to this exist throughout the lower and heavier land between Fruita and Palisade, where not only the fruit trees have died, as cited by Doctor Headden, but a part of the farm land has been ruined by the alkali rising. Some of the orchards in this section were badly affected before they were sprayed and none of the farm land has had any arsenicals applied at any time.

Many other specific instances could be cited of orchards that have been badly affected, or even entirely ruined, where no arsenical spraying has ever been done, but it does not seem necessary. The above facts seem to the writer to be sufficient to warrant the conclusion that it is impossible to identify this condition with any effect of arsenical sprays.

"Collar Rot" Killing Trees

The second condition described in the Colorado Bulletin is commonly recognized throughout the United States under the name of "collar rot," and apparently confines itself largely to the Ben Davis

apple, sometimes attacking the Gano and only rarely any other sort. In three or four places in Utah this disease has been alarmingly abundant. Mr. Lars Nording of Ilyrum, Utah, planted Ben Davis and Johnathan alternately in the rows in the orchard. . Within three years the Ben Davis began to die of "collar rot" and continued to die in this way, until now they are nearly gone, while none of the original Johnathan, nor those planted in the place of the dead Ben Davis, have been affected. No spraying was done in this orchard until nearly half of the Ben Davis trees were dead, and they have not died more rapidly since spraying commenced.

When the writer began his spraying experiments on the orchard of Mr. Smart a number of trees were found dead or dying of "collar rot," although the orchard had never been sprayed. These trees were all in a small section of the orchard and in this section other trees have died since the spraying was carried on, while the rest of the orchard has not been affected.

At Morgan, which is situated in a high mountain valley, a large number of Ben Davis were planted a few years ago, and before any of these orchards began bearing, and therefore before spraying had commenced, numerous complaints were made of the loss to their trees through this disease. The Central Experiment Station orchard contains about two acres of Ben Davis trees and when the first spraying was applied there were a number of trees dead and others dying of "collar rot."

The writer is fairly familiar with most of the orchard regions of both Colorado and Utah and has visited many of the sections of the Northwest, and in no case has he observed that there was any more loss from these causes in orchards that had been cared for and sprayed for years than in neglected and unsprayed orchards, which should, if Doctor Headden's conclusions are correct, be immune from these troubles.

The only conclusion that it seems possible to draw from the facts cited is, that arsenical poisoning cannot be the primary cause of either one of the above described conditions, and that therefore the main conclusion of Colorado Bulletin No. 131 is unwarranted. In this bulletin a third condition is mentioned, as occurring in a few cases, in which the sap oozes from the trees and forms a gummy or crystalline mass. This condition the writer has never seen and is therefore in no position to discuss.

It should be borne in mind in this connection that Doctor Headden was handicapped in his studies by the fact that the area under investigation is located several hundred miles from his laboratory, where

most of the work was done, and that his work in the field was therefore limited to one or two hasty trips. It is also well to remember that no experiments were carried on on fruit trees, the only attempt to test the effect of arsenic being made on green-house plants, and that the only trees known to have died from arsenical poisoning were the two that were killed by the soluble arsenite of soda, which we know would have killed them immediately if it had been sprayed on the foliage.

Mr. Carpenter, a former resident of this valley, is authority for the statement that the waters of the Grand River carry a considerable amount of soluble arsenic, received from the washings of mines and smelters located upon its head waters, and that at the present time arsenic can be found in all the soils of the lower end of the valley. This may possibly be the source of some of the arsenic found by Doctor Headden, rather than from the insoluble compounds used in spraying.

Where the Bulletin is Valuable

This bulletin, if rightly interpreted, contains considerable of value to the fruit grower. This interpretation, however, will be one of very guarded warning against intemperate and excessive as well as needless use of arsenicals. Thus construed it will only serve to strengthen the position taken by those who are urging the use of the new methods in spraying, worked out by the Utah Experiment Station and later confirmed by other workers. For in these experiments it has been abundantly proven that one single driving spray, if applied at the right time and in the right way, will do more to protect an orchard for the entire season than half a dozen sprayings in the old way.

Doctor Headden shows in his bulletin that even a small amount of ordinary alkali, or even of common salt alone, in the water will serve to render some of the arsenic of a spraying solution soluble, which serves to explain a number of things that have happened in Utah in the past. In one case an orchardist sprayed his entire orchard with Paris green and practically ruined his crop by burning, while his neighbors used the same brand of poison without serious injury. This particular orchardist, however, undoubtedly used strongly alkaline water from a surface well, while his neighbors took their water from an irrigation ditch.

In another case an orchardist used an approved brand of lead arsenate in spraying and killed everything that he touched with it. It was only a small job and it seems probable that the barrel used had formerly contained salt or brine.

This bulletin will also serve as a warning to those contemplating

planting trees in a strongly alkaline soil, even where the ground water is not ordinarily close enough to be considered dangerous, for if the rising alkali is capable of freeing the arsenic, it would not be very long before the soil would be so impregnated that it would not only kill the trees but render the ground unfit for any other crop.

The entire matter is one that calls for careful and exhaustive investigation, and for cautious and guarded statements of any kind until the results of these investigations are known. Hasty and ill-advised statements with reference to the purity of arsenicals have already done a great injury to the fruit industry in the intermountain region.

Conclusions

1. That the conditions described by Doctor Headden and attributed to the effects of arsenical spraying occur over widely distributed areas and have killed thousands of trees on which no arsenic has ever been used, and that therefore arsenical poisoning cannot be the primary cause of this trouble.

2. That the only trees positively known to have died of arsenical poisoning were the two to which a soluble arsenite was applied, a compound which no one has ever used for spraying purposes.

3. That the entire subject of arsenical poisoning is a matter for careful and exhaustive investigation and that any statements preceding that investigation should be of the most guarded nature.

4. That there is a possible danger in the use of even slightly alkaline waters in the application of spraying materials, and that there is a probability of danger from excessive spraying on strongly alkaline soils.

5. That those who are using the driving spray or contemplate using it may do so with the assurance that they are using the most effective and at the same time the least dangerous method possible,—if there should prove to be danger in arsenical spraying,—and that the best and most productive orchards in the West are the ones that have been the longest sprayed.

The following paper was briefly summarized by the author:

THE ALFALFA LEAF-WEEVIL

By E. G. Titus, *Logan, Utah*

For the past six years the alfalfa-growing territory in the vicinity of Salt Lake City, Utah, has been suffering from the attack of an

insect that is constantly increasing in numbers and continually widening its zone of injury.

Thorough examination of the surrounding region, especial attention being paid to native food plants, disclosed no attack upon these by this insect and I became convinced that the species was an introduced one. Mr. E. A. Schwarz recently examined specimens I brought to him and determined the species as *Phytonomus murinus* Fab., a European species not hitherto reported in this country. A single specimen had previously been determined by Mr. Schwarz as *P. castor* Leconte.[1] While we have not had the opportunity to compare the specimens with Leconte's type, his description so closely fits the European specimens and the Utah species that there seems to be no doubt but that we here have another instance of a species described from a wrongly assigned locality, Doctor Leconte giving "Canada" as the habitat, while the probabilities are strong that his specimen was of European origin.[2]

In Europe this species has long been known as a serious pest to alfalfa[3] and its advent into the western continent is of considerable importance, the number of insects already attacking the alfalfa being sufficient to make the question more than interesting to both the farmer and the entomologist.

The earliest definite report of injury in Utah that I have been able to trace is one that occurred in the spring of 1904, on a farm on the east side of Salt Lake City. Several acres of alfalfa in one field were at that time seriously injured, the first crop being one half lost and the second crop practically destroyed. While this was evidently the first serious loss, it should be considered that it must have taken several years for the insect to multiply in sufficient numbers to cause such an appreciable injury to the crop.

Distribution

The zone of injured fields has been constantly increasing, until at the present time it is known to occur from the hills northeast of the city to some distance south of the town of Sandy and eastward to the foothills. The western edge of the infestation until the past two years seemed to be the railroad lines running south from Salt Lake City. It has now passed this barrier and spread for some three miles westward along one line and a mile in another place. This makes a total area of almost 100 square miles of territory, in which there is probably grown about 2,500 acres of alfalfa. The extension to the west

[1] 1876: Leconte; The Rhynchophora of America, North of Mexico (Proc. Am. Philos. Soc. XV, Dec., No. 96, p. 126).

[2] 1882: Riley; Report 1, Entomologist. U. S. D. A., p. 172.

[3] 1838: Heeger; Isis, p. 980.

side of the railroad opens to its invasion a considerable number of very fine fields and will give the weevil an opportunity to work southward, onto some of the most fertile farming land in the state. The alfalfa raised in Salt Lake County is nearly or quite all used in the county, so that one serious means of spreading the insect is almost obviated. Where animals are being shipped from the county, for instance at the time of such events as the State Fair, abundant opportunity may be given for the insect to pass to some other uninfested district or even out of the state. Many of the fruit orchards are surrounded on one or more sides by alfalfa fields and the migrating weevils have been found concealed in fruit packages. The principal means of distribution of the weevil at the present time is that of migration by the adults in the summer and fall.

Life-History and Habits

The egg-laying period lasts for several weeks, due apparently to the long period over which adults are emerging from hibernation. The eggs are laid on the plant, and most of those found have been in or near the growing tips. The eggs are oval, pale yellow, .2 mm. long, darkening to a greenish yellow before hatching. I have reason to believe that some of the weevils lay some eggs in the fall near the ground on the stems.

The young larvæ are pale green, changing after their first meal to a green slightly paler than that of the leaves on which they feed. The color is so deceptive that unless larvæ are numerous it often takes considerable searching to locate them. When two thirds or more grown they are more easily seen. Usually while feeding the anal end is curled around a part of the leaf or bud in the same manner as in some other species of the genus. At this age they are easily disturbed and often fall to the ground before the plant has been touched. There are at least four distinct stages in the life of the larva, each lasting from eight to ten days. The full-grown larva is 4 to 6 mm. long and has a distinct broad white dorsal stripe.

The full-grown larva drops to the ground and among the fallen leaves or at the base of the stems spins an open-work lace cocoon much finer than the case of *P. punctatus*. After several days, depending somewhat on the weather, a greenish white pupa is formed and in from 10 to 14 days the adult weevil appears. After two or three days it hardens, turns brown, and then cuts its way out of the cocoon, crawls up a stem and begins feeding.

The beetle varies from 4 mm. long in the male to 5 or 5.5 mm. in the female. When freshly emerged they are brown with a distinct darker

line extending centrally down the elytra. Head and pronotum are finely, closely pubescent with gray hair, that on the head sometimes extending quite to the tip of the beak; prothorax slightly longer than wide, narrower in front than behind, widest in the middle, rounded on the sides and densely finely punctured. There are two longitudinal stripes of brown hairs or rather fine scales, separated in the middle by a narrow gray line; the sides of the prothorax are covered with dense gray hairs; elytra ⅓ wider than the prothorax, oval, but with sides nearly parallel, humeri rounded, the striæ are distinctly punctured. The elytra have rows of fine gray setæ alternating with hair-like scales (each scale being deeply cleft so that it appears as two hairs); there are some fine darker hairs present in such a manner as to present small spots of black. Antennæ with first funicle joint much longer than second; last joint of funiculus separated from club. Legs dark brown with numerous gray hairs present on the femora and tibiæ, tarsi more sparsely pubescent; antennæ slightly pubescent.

After a short time many of the specimens lose considerable pubescence and appear very dark or even quite black, almost giving the appearance of two species present in the field. The males are slightly narrower than the females in proportion to their size.

All stages of the larvæ, the cocoons and fully developed beetles, have been found in one field during the latter part of June and throughout July. Larvæ not half grown have been taken on September 14. These probably were belated larvæ that had failed to develop as rapidly as the others, since there is no present evidence of there being more than one brood of the weevil.

A large percentage of the weevils soon begin to migrate, moving in almost every direction from the field. I have been told that in the late fall, on very warm days, they have been seen flying, but so far I have been unable to get one to fly, nor have I seen any on the wing. They are strong walkers, untiring and steady. I have found them scattered all across a forty-acre piece of uncultivated land, moving away from the alfalfa field on one side and many of them eventually reaching an uninfested field on the opposite side of the "forty." Others fall into the irrigating ditches and are swept onward, and at times into a field, where they may secure lodgment on leaves or the stems of the plants. Once they obtain solid footing on they go until they reach a field where food plants are present or until night drives them to shelter. The weevils are quite susceptible to warmth. Even the passing of the sun behind a cloud will drive them to shelter. They go into hibernation early in the fall, rarely coming out and feeding until

spring has opened up. Almost any place, from a haystack to dead leaves along an irrigation ditch, will attract them.

Many live over winter in haystacks, either coming directly on foot from the surrounding infested area or being carried on hayracks during August. Others find their way to the edges of the field and crawl beneath dead leaves or grass along fence rows and ditch-banks. Still others, especially in old alfalfa fields, crawl down between the bases of the stems of the larger plants and remain well protected.

In June I have seen large numbers of the larvæ on hay racks after unloading at the stacks or barns. While the larvæ will never be freely distributed in this manner there are always some that are so nearly full grown that they will pass through the rest of their changes in the protection of the stack or in some sheltered spot near where they have dropped from the wagon.

Coming out of hibernation early in the spring, they feed upon the young leaves to a very slight extent. Eggs are soon laid and where the infestation is at all heavy (that is, where they have been present at least one year) the first crop will be nearly ruined. Cutting this at the usual time shakes the larvæ to the ground and as they are not nearly full grown, they crawl back to the plants, and the result is the second crop has no opportunity to grow, every bud and leaf being cut off until the larvæ are full grown and pass to the ground to pupate.

Injuries

The weevils feed on the stems, rasping off a small amount of tissue, and on the leaves and buds. They never do much harm, but where plentiful the injury can be readily detected. The young larvæ feed in the growing tip, on and between the young opening leaves, buds and even flowers. As they grow older they work their way on the larger and older leaves and often completely defoliate the plant. Where young larvæ are compelled to feed on the older leaves they only skeletonize the foliage. This often gives the plant the appearance of having been frosted, the color, however, being whiter than that following frost injury.

Remedies

No very satisfactory means has yet been devised for handling the insect. It can be checked and its spread somewhat limited by several operations. Different conditions and soils will require different treatment. Poisoning can scarcely be resorted to since it means an entire loss of the crop and would be a costly measure. Burning over the fields in the fall before it is cold enough to drive the weevils to hibernation kills a large number. By scattering straw over fields in the spring

as soon as the larvæ are seen and then burning off the field many eggs and young larvæ will be destroyed and many adults crippled. The alfalfa quickly recovers from the burning and a good crop is assured. Seriously infested fields may be deeply ploughed in late May or early June, in this manner killing practically all the larvæ and eggs and turning under good fertilizing material. While this is one of the surest methods of handling the insect, it will be hard to get the average farmer to consent to such measure, unless it is an old piece of lucerne that will soon have to be broken up. Any means used in the field should not be begun until all the weevils are out from hibernation.

Clean cultivation around the edges of the fields and on the ditch banks, especially burning up all trash after the weevils have gone into hibernation, will aid considerably in controlling them. Unfortunately the weevil is beginning to turn its attention to sweet clover (*Melilotus alba*) and to yellow clover (*Melilotus officinalis*) and red clover (*Trifolium pratense*), the first named plant being very common along roadsides and ditch banks in some localities.

No parasitic insects have been bred from any stage of the species. Horned toads, swifts, and the common garden toad all feed upon the weevil and the latter also eats the larvæ. Chickens will pick up the weevils and if the larvæ are numerous enough will also feed upon them, but do not seem to especially relish the food. A field mouse was found with remains of a weevil and it is probable that some birds will attack them.

Mr. Schwarz: Mr. President, five European species of *Phytonomus* have been imported into this country, three of which, including the alfalfa leaf beetle, are of economic importance. The well-known clover leaf beetle (*Phytonomus punctatus*) was originally introduced into the state of New York. A second species, *Phytonomus nigrirostris*, which for many years was known only in the northeastern states, has suddenly made its appearance in the vicinity of Washington, D. C., and threatens to become as injurious to clover as the original clover leaf beetle. Its natural history is now being worked out by Prof. F. M. Webster.

Insects belonging to the circum-polar fauna or insects that have been imported from Europe into our northeastern states never spread southward, and such invasions as those of *P. punctatus* and *P. nigrirostris* indicate that these particular species were imported from Europe into regions lying south of the transition fauna.

I predict that the alfalfa leaf weevil will spread rapidly to the al-

falfa regions of the West and that it is likely to prove enormously destructive to this important crop.

President Forbes: If there is nothing further on this subject, the next on our program will be by Mr. E. P. Taylor.

AN EXPERIMENT IN THE CONTROL OF CURCULIO ON PEACH

By Estes P. Taylor, *Mountain Grove, Mo.*

The greatest insect problem confronting the peach growers of the Ozarks is the prevention of injuries from the curculio (*Conotrachelus nenuphar* Hbst.). From reports this is also the paramount question with peach growers throughout the whole of the Mississippi River Basin, from the northern limits of the peach on into the great peach-growing district of the south. Nor is the middle-west and south alone the territory involved, for many eastern states find losses from this source quite as heavy. Making no exception for brown-rot, peach-scab or other fungus disease, it may, in Missouri, excepting perhaps in those sections into which the San José scale has been introduced, be easily accorded first rank among all spraying problems of the peach grower.

This condition makes the problem today one of the greatest economic interest to peach growers and its solution in a practical way for the benefit of those interested in peach culture is one of the most productive fields now open to the economic entomologist.

The possibility of controlling curculio in apple orchards by means of arsenical sprays cannot now be doubted. This was shown to be possible in experiments conducted nearly twenty-five years ago in Illinois by Doctor Forbes, the present chairman of this Association, and at the meeting held at Philadelphia in 1905 further definite experiments were reported, showing remarkably successful results in this direction. Although apple growers have also employed with varying success other methods of control, the practical results to be derived from arsenical sprays has gained much favor among them within the past few years and there can be no question that spraying is the most important single operation which can be employed to reduce its injuries upon apples.

Our peach growers have, upon the other hand, entertained serious doubts as to the possibility of destroying the beetles in the peach orchard by means of arsenicals. Although here and there tried, summer spraying of peach cannot be said to be anything like a common practice. We must grant at the outset that much of the hesi-

tancy of peach growers to use arsenicals upon peach trees has been due to their fear of doing injury to the foliage or fruit, a fear which in the past, with the use of Paris green, has been well grounded. The advent of arsenate of lead as an insecticide, with its increased adhesiveness and greater safety to the tree, makes the practicability of peach-spraying for this pest seem more promising.

An experiment conducted by the writer this season has given additional and convincing evidence that injury from the curculio can be largely prevented upon peach by proper spraying with a dilute solution of arsenate of lead, and this at a cost and with results which makes the treatment a practical operation to every peach grower in the territory where this insect occurs as a pest. Although this report is intended only as a preliminary one upon this subject and is not presented as a final recommendation to growers, it is thought that a brief account of the investigations thus far conducted and of some of the facts gathered would be timely at this meeting. The results thus far may be also of interest since the plan is, in the main, similar to one being carried out by Professor Quaintance of the Bureau of Entomology.

The Spraying

The spraying experiment was conducted upon a block of 1195 six-year-old Elberta trees belonging to the Olden Fruit Company at Olden, Missouri, and forming a portion of one of the most extensive fruit plantings in the Ozarks, comprising in all about 1,800 acres. The spraying was done with a gasoline power outfit. The spray used was Swift's arsenate of lead, costing about 12 cents per pound, and the average amount required for a very thorough application was about two gallons per tree, or only about one cent per tree per application for insecticide and only between two and three cents per tree per spray when cost of application was also included.

Upon all of the principal plats, containing from 32 to 249 trees, 2 pounds of the paste lead were used per 50 gallons, with the addition of 4 pounds of quick lime for the purpose of neutralizing any free arsenic which might happen to be present. To determine the effect of an increased amount of the lead arsenate upon the tree, a number of small plats were given varying amounts of arsenate of lead, both with and without the addition of lime. The season covered by the spraying was one of unusual rainfall. In the month of April 9.06 inches and in the month of May 10.24 inches were the recorded precipitation at the orchard, while the month of June gave 15 days in which rain fell. Not only were these conditions unfavorable for the best results but several damaging late frosts reduced the crop to a very light

yield, a condition naturally expected to favor heavy infestation by the beetles present.

The Results

A record of the windfall peaches was secured by collection of fallen fruit under trees carefully selected at the beginning of the experiment from the central portions of each plat, so as to minimize the complications in the results otherwise likely to follow from the spreading of the beetles from one block to another. From these indicated trees the ripened peaches were picked on July 31st.

At the time of the picking of the fruit 86% of the peaches in the unsprayed block had fallen to the ground, 94% of them being wormy, as determined by cutting open each peach in making the examinations. Of the few remaining peaches left upon the unsprayed trees at picking time every one was found to have been injured by curculio.

In one plat which received three applications of lead arsenate, the first at the time the petals had fallen, the second thirteen days later, when the "shucks" or dried calyces were shed from the peaches, and the third eleven days later, gave 87% of the picked peaches free from curculio injury.

Another plat, which received only two applications of lead arsenate, the latter two as given in the plat just cited,—one when the "shucks" were shed and another eleven days later,—gave 89% of the picked peaches free from curculio injury.

In still another plat, which received three sprayings of arsenate of lead, one when the "shucks" shed from the peaches, another thirteen days later, and a third eleven days after the second, yielded 94% of the peaches free from curculio, and the best results of all.

It will be seen that a most remarkable improvement was secured by the spraying in the prevention of damage by curculio; a much greater difference than I had expected or even hoped for at the beginning of the experiment.

A secondary beneficial effect was secured in the control of brown-rot and peach scab. In the unsprayed block 61.3% of the picked peaches counted showed brown-rot infection and in practically every case the infection surrounded a puncture made by a curculio. In the plats where curculio was controlled most successfully brown-rot caused practically no damage. In one case where the curculio injury was reduced to 13.3% the brown-rot was reduced to 3.8%, and in another plat where curculio was reduced to 11.4% brown-rot was reduced to 2.3%.

It is almost certain that the adult curculios convey the spores of

brown-rot upon their feet or bodies by crawling first over brown-rot "mummies" or infected spots and then to non-infected peaches, where they may establish newly-infected areas by making new punctures or passing over old ones. This relation of the curculio to brown-rot infection, many times noted by entomologists, is worthy of further careful observations.

Arsenate of lead undoubtedly also possesses some fungicidal properties. Upon apples it has shown such action and in this experiment upon peaches a decided decrease in the amount of peach scab was noted in the sprayed portions over those left untreated.

An additional gain in improved color of fruit was also secured, which alone was enough to almost repay the cost of spraying. The peaches from the sprayed blocks where the best results had been secured in controlling curculio were of a beautiful bright red color, some very dark in fact, making them extremely attractive and adding greatly to their market value. That this color was the effect of the lead arsenate was plain, due in part perhaps to greater exposure of the peaches to the sun by a diminution of the foliage on some trees by spray burning, but chiefly due to direct chemical or physiological action of the arsenate of lead upon the tissue of the peach.

In some plats treated the action of the spray was severe enough to cause burning of foliage and fruit and to cause uneasiness as to the outcome. A spray of lime water was given at one time to some plats to arrest this burning action. In some plats, where the very best results were secured in controlling the insect, some peaches showed unmistakable indications of burning upon the foliage, peaches and small twigs, though from recent observations upon these trees this injury does not appear to have been serious enough to the twigs to materially affect the prospect for fruit upon the trees the coming year.

We may sum up the results of this preliminary experiment as showing the complete possibility of the prevention of the majority of curculio injuries by arsenate of lead sprays but leaving some questions of safety to the tree from the spray still unsettled. Some important points bearing upon this were, however, brought out by the experiment, which will be valuable in the investigation continued upon the subject next year. Some of the points thought to be established and worthy of mention are:

1. The increase of lead arsenate in the spray formula increases the danger of injury.

2. The addition of lime to lead arsenate for peach spraying reduces the danger of injury.

3. The use of lead arsenate as dilute as 2 pounds per 50 gallons with

the addition of 4 pounds of lime is not always safe for peach spraying.

4. Early applications of lead arsenate upon peach are less likely to cause injury than those of the same formula applied later.

5. Lead arsenate used upon peach should be practically free from uncombined water soluble arsenic and should contain a maximum amount of arsenious oxide.

Spraying Suggestions

The following is a scheme proposed for peach spraying for the spring of 1909:

First Spray.—Immediately following the time the "husks" have dropped from the small peaches, which will be when Elbertas usually measure from ⅓ to ½ inch in diameter. Use one pound of guaranteed arsenate of lead, 2 pounds quick lime to each 50 gallons of water, applying the spray as a rather fine mist spray, using no more material than necessary to thoroughly coat the surface of every peach upon the tree. The pubescence over the peach will hold the spray and upon drying will leave the poison evenly distributed. This spray will be at about the time the first food punctures are being made.

Second Spray.—About ten days or two weeks following the first application repeat the spray, using the same formula applied as a mist and with the same thoroughness as before. This spraying will be done when numerous punctures, both food and egg, are being made.

Third Spray.—About ten days following the second give a third application in the same manner as before. At this time Elbertas will ordinarily measure 1 to 1⅓ inches in diameter.

These times for application being based upon development of the peach and with no reference to specific calendar dates should be applicable as guides for the proper timing of sprays for peach growers living in any section.

The treatments, it will be seen, are suggested at a time early in the development of the peach, at the time when the first and when the majority of the food punctures are being made. The experiments this year did not indicate that arsenical sprays applied to peaches previous to the dropping of the dried calyces gave results of sufficient value to justify them. On the other hand, the destruction of the hibernating beetles early prevented egg deposition and intercepted the development of the generation emerging later. Of the three sprays recommended, the first and second are the most important. Peach trees appear to be more susceptible to injury when sprayed later than early. The time when the most adult beetles may be destroyed, fortunately for

the fruit grower, coincides with the time when less injury is apt to be done to the trees by the spray.

This scheme of suggested treatment, it will be seen, reduces the amount of arsenate of lead from the formula used in the experiment. It seems probable, at least, that this amount of arsenical will bring about successful results. Orchardists in some sections are using only this amount of poison in their formulas against codling moth.

None but the best grades of lead arsenate should be used and every package purchased should be required to bear a certificate of analysis, showing quality at least up to the standard established by the national insecticide bill favored by this body for enactment before the coming session of Congress.

Mr. Slingerland: What is your theory in regard to the way the poison kills? Does it kill the beetles, or don't they like the spray?

Mr. Taylor: My belief has always been that it kills the beetles, as it is not supposed that arsenate of lead is a very strong repellent.

Mr. Quaintance: The Bureau of Entomology has been interested in this subject for several years, and I am glad to hear Mr. Taylor's paper. It confirms results we got three or four years ago, especially as to the high percentage of peaches which may be protected from the curculio attack. As to the use of arsenate of lead on peach trees, especially as to recommending its use to peach growers in the southern states, I should think it would be unwise without explanations. The effects of the lead on peach varies from year to year, and I think I have never seen the same results twice. Some years there is but little if any harm, and the next year the foliage is badly injured and much of the fruit may fall. Tests of home-made arsenate of lead show but very little difference. This question of injury has been under investigation by the Bureaus of Chemistry and Entomology, and it turns out that arsenate of lead, after being applied to the trees, undergoes decomposition, and the presence of lime or its absence, while entering materially into the question, does not prevent burning. I want to urge that the recommendation as to the use of arsenicals on peach in the southern states be done with caution and that the grower know the risk he is taking.

A Member: What time do you spray, Mr. Quaintance?

Mr. Quaintance: Just about as Mr. Taylor said, beginning as soon as the petals are down.

Mr. Taylor: I would like to ask Mr. Quaintance if he has seen severe injury from the best grades of arsenate of lead where only a

pound of lead was used, with two pounds of lime added per 50 gallons?

MR. QUAINTANCE: Yes.

MR. SKINNER: I have been much interested in this paper, and I think Professor Slingerland's question is very important, as to whether the arsenate of lead is repellent, or whether it acts as a stomach poison. If it is a repellent it seems to me that it would be possible to use some other material that would have the same effect without any injury to the foliage. It has seemed to me that one of the great troubles about economic entomology is that too much reliance is placed on arsenicals,—perhaps I should not say "reliance." From the standpoint of economic entomology there are so many other chemicals that can be used. This Association has demonstrated that the men working on these lines are largely investigators, and at least have some time to devote to investigation, and these investigations of problems are the ones that to me, personally, are extremely interesting. It seems to me that that particular point is one of very great interest, whether these insects are deterred from the fact that the material that they eat is a stomach poison or whether it repels them in some other way, and I am quite impressed with this idea of the subject, and I sincerely hope that the men who have the opportunity of studying these matters will take up that point and report on it.

MR. HINDS: I would like to ask in regard to the feeding of the plum curculio after the crop of fruit has been gathered. Is it practical to apply arsenical poisons after the crop has been removed and when the foliage is stronger than earlier in the season?

MR. TAYLOR: Mr. President, I made some jarring records this summer and found that there were very few beetles on the trees at that time. This is not conclusive evidence that some feeding might not take place, although the number of beetles secured was very small.

MR. SCHWARZ: Mr. President, it is possible that there are two species of *Conotrachelus* that attack peaches. Many years ago, while visiting Prof. H. A. Morgan at Baton Rouge, Louisiana, he showed me specimens of *Conotrachelus anaglypticus* which had been bred from peaches. It would be worth while to try to ascertain the breeding habits of *C. anaglypticus*, as it is one of the common weevils in eastern United States. I do not think that Professor Morgan has ever published this interesting observation.

PRESIDENT FORBES: The next paper will be by Mr. Hinds.

CARBON DI-SULFID FUMIGATION FOR GRAIN INFESTING INSECTS

By W. E. Hinds, *Auburn, Ala.*

It is now fifty years since Doyere discovered that the vapor of Carbon di-sulfid was an effective agent in destroying grain-infesting insects. In 1876 two other French investigators, working particularly with the grape Phylloxera and a few other species of insects, announced that "one part of Carbon di-sulfid vapor in ninety parts of air killed all insects in a few seconds, while one part of the gas in 254 parts of air was fatal in 75 minutes."

An examination of the literature of the United States Bureau of Entomology and the state experiment stations discloses no extended experimental work in this country. This strongly suggests that the recommendation which has been most commonly made, i. e., for the use of "one pound of the liquid Carbon di-sulfid for each one thousand cubic feet of fumigated space during a period of twenty-four hours," has been based upon the report of the two Frenchmen, Cornu and Mouillefert, and that their conclusions have been accepted with little question and little subsequent confirmation. The possible injurious effect of the gas upon the germination of seeds seems to have attracted more attention from station workers in the United States than has the question of killing insect stages. The most extended experiments of which we have found record in this country were conducted by Osborn and Mally in Iowa, Webster in Ohio, and Pettit in Michigan. It is quite possible that other reports of importance may be found in papers which we have not yet been able to examine.

In most of the records which we have seen it is stated that the gas was used at a "saturated atmosphere," but no consideration seems to have been given to the effect of temperature upon the actual amount of Carbon di-sulfid contained in a "saturated atmosphere." According to data furnished by the United States Bureau of Chemistry several years ago, the amount of Carbon di-sulfid in a saturated atmosphere varies with the temperature as follows: At 50 degrees F., 53.5 lbs. of CS_2 ; at 59 degrees, 64.6 lbs.; at 68 degrees, 77.6 lbs.; at 77 degrees, 92.4 lbs., and at 86 degrees, 109.3 lbs. Thus at 86 degrees slightly more than twice as much of the gas is required to saturate the air as is needed at 50 degrees. Obviously the air temperature at the time of treatment makes a most essential difference in the effective strength of the gas and should be taken into account in all careful experimental work, although it seems to have been disregarded heretofore in the treatment of both seeds and insects. At a temperature of 72 de-

grees the dosage usually recommended will produce but about 1.2 per cent of a saturated atmosphere, while at 50 degrees it will produce about 2 per cent of a saturated atmosphere.

Another consideration involved in interpreting the results as given by various writers for the treatment of insects is the possible temporary asphyxiating effect of the gas, which does not produce ultimate death. In all of the records where we have found mention of the time of making examination to determine the effect of the treatment, it seems that the examination has been made immediately at the close of the treatment. In the absence of statements to the contrary, it seems reasonable to assume that this was probably the case in nearly, if not quite all, of the experiments. Our own observations have led us to believe that such immediate examinations are very liable to lead to incorrect conclusions. We have made it a practice to preserve the apparently dead specimens in each lot as well as the living for continued observation following the experiment. The first effect of the gas is to stupify the insects and this occurs some time before actual death takes place. Thus it is quite possible that in a case where all of the insects appear to be dead on an immediate examination, a large percentage of them will recover upon exposure to the air or under the gradual dissipation of the gas. It may require from one to two days for them to resume normal activities. Some of those which thus become active may indeed be so seriously affected that they cannot recover the ability to feed and so ultimately die as a direct result of the treatment. The standard which we have therefore adopted for our own work is to count as "living" only those insects which subsequently resume their normal activities, either in feeding and in reproduction, or in development and transformation. If they do not thus recover it is evident that the treatment is ultimately and practically effective.

It is not our purpose in this paper to review the work of other entomologists with Carbon di-sulfid, or to question specifically the report of any experimental results. Nor shall we attempt to compare the records of our own experiments with those of any other workers. We have called attention to the apparently foreign origin of our most commonly accepted recommendations for its use and to a few of the many chemical, physical and entomological considerations which seem to have an important bearing upon the interpretation of previously accepted results, merely to show that from our point of view the entire subject of the use of Carbon di-sulfid as an entomological fumigant is still an open question, so far as the scientific points involved in its use are concerned, and that this therefore constitutes an important field, deserving the most careful and thorough investigation possible.

The work which we have yet done in this field at Auburn, Alabama, is but a beginning in line with an investigation which promises to require several years for its completion. The general plan and purposes of the investigation have been outlined in a preceding paper. In this work I am being assisted by Mr. W. F. Turner, to whom is due much of the credit for the execution of the work. Thus far our work has dealt only with corn and cow-peas and some of the insects infesting them. We consider the results as only tentative, but still as of sufficient suggestiveness to be worthy of your consideration. The principal points investigated thus far are the effects of the gas upon the germination of treated corn and upon the life of adults and immature stages of *Calandra oryza, Silvanus surinamensis* and *Bruchus chinensis.*

The investigations of Hicks and Dabney, as reported in Circular 11, United States Department of Agriculture, Division of Botany, indicated that field corn is one of the seeds most susceptible to injury by gas treatment. In their experiments the germination of corn treated for forty-eight hours with a saturated atmosphere of gas was reduced forty per cent, or from 94 per cent in the check to 54 per cent in the treated lot. A twenty-four hour treatment, however, resulted in no injury.

In our tests of the effect of the gas upon the germination of seed corn we have used two varieties. Number one is known as "Henry Grady" and is a white, dent corn, which is much subject to weevil attack. Number two is known as "Station Yellow" and is a yellow flint corn, which has been selected for several years to secure greater resistance to weevil attack. Each of the twenty-five lots tested included one hundred seeds and the seeds were taken from several ears so as to make the lots as nearly uniform as possible. One lot in every five was used as a check. The weight of each lot was determined before treatment and immediately after being removed from the gas. The strength of gas used was a "saturated atmosphere" at the room temperature, which was about 68 degrees. The temperature is continuously recorded by a thermograph. The actual strength of gas used was about seventy-five pounds of CS_2 per 1,000 cubic feet.

In two lots the seeds were soaked in water for one hour before being placed in the gas. In this time the seeds gained 11.2 per cent on their original weight. It is sufficient to say that all seeds were killed in these two lots in treatments of forty and seventy-two hours. Other tests have shown that corn will absorb in a two minutes submersion in water as much moisture as it will subsequently lose in two days in

air under an atmospheric humidity of about 65 to 70 per cent of saturation.

In a comparison of lots of seed treated alike before and during the gas exposure, but in which one series was placed in water for soaking preparatory to germination immediately after being removed from the gas and weighed, while a parallel series was allowed to air for twenty-four hours before they were germinated, there appeared to be a greater injury among the seeds which were not allowed to evaporate their absorbed gas before being soaked in water. In the check lots the average percentage of germination was 91; in the treated seed which was aired it was 73, while in that not allowed to air it was but 65 per cent. In both series the seed treated from four to six days germinated practically as well as did that treated from one to three days. The increased length of treatment did not result in a proportionate injury to the germinative power of the seeds. In no case was the injury as great as that reported by Hicks and Dabney for a forty-eight hour treatment. It is evident that the proportion of water in the seed at the time of treatment has much to do with the possibility of gas injury. We have not concluded our investigations along this line.

The second part of our work is concerned with the effects of the gas upon insects either as adults outside of the seed or as immature stages within the seeds. In this work a saturated atmosphere was used in bell-jars, as with the germination experiments. The minimum time for each series of experiments was taken as the time at which all individuals in a lot appeared to have succumbed to the effects of the gas and remained quiet. From this time on each lot was given an added exposure of five minutes. After the treatment the insects were left undisturbed to air for about eighteen hours and were then carefully examined. All specimens, whether apparently dead or evidently alive, were preserved for further continued observation and in numerous cases it was thus found that insects recovered activity after from one to two days, during which they had showed no sign of life.

In the experiments with adults of *Bruchus chinensis* more than fifty per cent was killed in twenty minutes in the gas and about ninety per cent in thirty-two minutes. Among over two hundred adults treated for forty-five minutes only two ever moved after being removed from the gas, and these did not recover sufficiently to feed. With the immature stages, under similar conditions of treatment, practically all stages were killed in a thirty-minute treatment. The effects upon the eggs of this species and upon the germination of cow-peas have not yet been determined. This species is much more easily killed than either of the other two which have been studied particularly.

In similar tests with *Calandra oryza* about seventy per cent of the adults were killed in from thirty to forty minutes, and about eighty-five per cent in from forty to fifty minutes. Among 209 adults exposed for sixty minutes but five showed subsequent signs of life, and one of these survived for three days but did not feed. This individual showed no sign of life until after having been out of the gas for about thirty-six hours. With this species also all treatments of more than one hour's duration resulted in complete destruction of the adults, and also of all immature stages, while nearly all of the latter were killed in the fifty-minute treatments.

By far the most resistant species thus far tested is *Silvanus surinamensis*, which is associated with Calandra and with *Cathartus gemellatus* in nearly all of the corn ears examined to date. While some of these adults were killed in treatments of thirty minutes, or even less, an average of but sixteen per cent was killed among over 200 specimens exposed for intervals of between thirty and sixty minutes. Among 140 adults exposed for periods ranging between 70 and 100 minutes the death rate was but 55 per cent, but among 200 adults exposed for 110 and 120 minutes all were killed.

The strength of gas actually effective in these experiments was between 70 and 80 times that evolved in a treatment using but one pound of the liquid to 1,000 cubic feet of space. As adults in these three species remained active in this strong gas for from 17 to 30, or even more, minutes, we may be excused for doubting the reliability of the statement of the French investigators to the effect that "in an atmosphere composed of one part of CS_2 vapor to 90 parts of air all insects perished in a few seconds." That strength of gas is not likely to be over one-sixtieth of the strength used in our tests in which the three species mentioned were active for more than twenty minutes on the average.

In an actual test of the application of the di-sulfid at the rate of two pounds per 1,000 cubic feet at a temperature of 55 to 60 degrees, for a period of twenty-four hours, the adults of both Calandra and Cathartus appeared to have been hardly disturbed. There was no indication of any mortality having resulted and when removed from the fumigation chamber at the end of the treatment the adults appeared to be moving and feeding normally.

Mr. Hinds: I trust that we may have time for some little discussion along this line, as we shall certainly appreciate the suggestions that will be brought out thereby. I had intended to give a few of the

results which we have already attained in our work, but, owing to the lack of time, will present the paper for publication in the proceedings.

Mr. Sanderson: Mr. President, I certainly am glad to learn of this investigation, because I have been interested in this matter for a long while. We need information on the diffusability of these gases. I did a little work on that some years ago, and I found that my friends who were working in physical science had very little information on that subject which was adaptable to our work, and I imagine there is very little work in pure physics along this line.

I am also interested in Doctor Hinds' presenting this outline to the Association. It has been a feeling of mine for a good while that we could save each other an enormous amount of work if we knew what each was doing. I have had occasion many times to benefit immensely by the plans submitted by other men. It seems to me that these meetings would be much more profitable if we could have more plans offered and short opportunities for discussing them. If time did not permit of that, if each of us would give a brief outline of the projects under way for the next year and the scope of the projects, it would be of immense value in getting together those workers interested along the same lines, so that by correspondence we could get our work in such shape that it could be compared in different parts of the country.

Mr. H. T. Fernald: Doctor Hinds' outline is very interesting to me because for the past four years in my laboratory various students have worked along similar lines, and the paper which I have withdrawn from the program today was practically a summary of that work as bearing on the treatment of certain greenhouse crops,—tomatoes and cucumbers. We have found that humidity is a very important factor, and, strange to say, so far as we have gone, we have found that with tomatoes a rather high humidity was favorable to successful results, while rather low humidity in the other case was preferable.

We have found that the most advantageous treatment with hydrocyanic gas is influenced by the amount of light, even at night. Of course, day-time experiments were unsuccessful. We also found that moonlight night experiments were, in some cases, very dangerous, and that the darkest nights gave the safest results. These results were totally unexpected, but the experiments were carried far enough to show that light entered into the subject.

From the results of these four years' work I have gradually come to the conclusion that there is no factor whatever that can enter into a series of experiments which is too small to be ignored, and I would urge Doctor Hinds and any one who is going into these experiments to

be as certain as possible that no possible factor has been overlooked. Some small thing may turn out to be most important in the end.

MR. SKINNER: I don't know that this is exactly in the line of discussion of this paper, but it is simply a thought that has interested me in regard to economic work for some time, and men like Doctor Fernald, who have student investigators, have an opportunity to take up these studies. It seems to me that with regard to stored grains we can get rid of a good many insects. I would suggest a very simple method, and that is to replace air in proper receptacles; for instance, with a material like carbonic acid. It seems to me that it would be a most effective method, with almost no expense, and it would obviate the question of poisons as we ordinarily understand them. I have made some investigations of these points in a very limited way, and it seems so practical and so self-evident that I bring it before the Association in the hope that some who have the opportunity will take it up. It is such a simple matter to deprive insects of air, and it is so effective when it is done, in spite of what some people say to the contrary, that I am in hopes that the subject may be investigated.

MR. SLINGERLAND: I was very glad that Doctor Fernald spoke about the effect of moonlight. I came across a case some years ago in the Hudson River valley, where they grow violets very extensively under glass. A violet house was fumigated with the usual dose and one night the plants were severely injured by the gas. The only unusual factor that might have caused the trouble was that it was a bright moonlight night.

MR. GOSSARD: A few years ago, in experimenting with fumigating orange trees in Florida, our chemist made some tests to determine the amount of gas generated by combining the acid and water just before the cyanide was thrown into it, and it gave a considerably larger volume of gas than if the acid and water were put together and had time to cool before they were taken into the field. We could save six or seven cents per tree in case of large trees by observing that point.

MR. J. L. PHILLIPS: The past season in Virginia has been rather dry, and some of the nurserymen have complained of injury from fumigation. In one case I found that the stock had been exposed to freezing weather for about twenty-four hours, and this might have caused the trouble in this case. We have had several other complaints this year, and I would like to know whether this has occurred in other states.

The method of the diffusion of gas has seemed to us to be very important, from the fact that some of the nurserymen pack the stock so closely in the house that it is impossible for the gas to circulate. This

difficulty might be overcome by having a slatted arrangement around the inside of the house for the circulation of the gas.

Mr. Burgess: Mr. President, I want to make a statement in regard to the importance of the diffusion and the penetration of gases, especially concerning hydrocyanic acid gas. This summer I had occasion to fumigate a large warehouse in which was stored large quantities of shelled peanuts in sacks, which were badly infested with Indian-meal Moth (*Plodia interpunctella*), and the treatment was not altogether satisfactory. A large number of worms on the outside of the sacks were killed, but there were a good many live ones on the inside, where the gas did not penetrate. A double charge of cyanide was used and the room was kept closed for about eighteen hours, but still the fumigation was not entirely satisfactory. I think we need to know a good deal more about the penetration and the diffusion of these gases before we can attempt to use them successfully in a great many cases.

A Member: Respecting Mr. Phillips' statement and some others about the diffusion of gases, we have been able to get very successful results by putting electric fans in our fumigating houses. By placing the fans in such a position in the house that they stir up the air the gas is sent through the house.

Mr. W. D. Hunter: I should like to say that it seems to me that Doctor Hinds could add very well to the outline he has projected some investigation of other fumigants. Of course, he has hit upon the two, hydrocyanic gas and carbon bisulfide, because they are in common use. They are used by everyone everywhere, but, although he has plenty to do in the investigation of those two gases, it seems to me he might take up other gases. The point I am about to make is suggested by what Mr. Burgess mentioned with reference to the penetrating power of gases. Take the derivative of sulfur, known as Clayton gas, sulfur dioxide, which has, I think, the most remarkable penetrating power of any gas ever used. Investigators have succeeded in killing the germs of typhoid fever in a steel cage, placed in a compressed cotton bale, with a density of fifty-six pounds to the cubic foot. The compressed bale is as hard as wood, and these germs placed in the steel cage have been killed by the gas. I understand now that a very cheap and simple method of generating this gas has been perfected by one manufacturing company. They have a little affair that you can push around that doesn't cost much, and it simplifies the whole matter greatly.

President Forbes: I think Doctor Hinds' expectations must have been very nicely fulfilled by this very interesting and valuable discussion, which has run, perhaps, as far as we can allow it. The last paper on this group of subjects is by Mr. Symons.

SUMMARY OF FUMIGATION AND DIPPING EXPERIMENTS

By Thomas B. Symons, *College Park, Md.*

The desire to verify the results of the rather limited amount of work conducted by other investigators on the effect of various strengths of hydrocyanic acid gas with different exposures upon tender peach buds and at the same time to demonstrate to the nurserymen of the state the great importance of fumigating all propagating stock to prevent the introduction and dissemination of the San José scale prompted us to conduct a series of experiments with this gas during the past three years. The experiments in fumigating buds were conducted at two nurseries in the state under as normal conditions as possible. In addition a series of tests with salable nursery stock badly infested with San José scale was conducted to observe the effect of the gas upon the scale and trees. The trees were fumigated at the nursery and planted out on the Experiment Station Farm. I cheerfully acknowledge the assistance of Messrs. A. B. Gahan, Geo. P. Weldon and L. M. Peairs in conducting these experiments.

1. On September 7, 1906, 505 peach buds were fumigated with 0.40 and 0.50 grams cyanide per cubic foot and exposed to the gas for 30 minutes and 805 peach buds were fumigated with the same strengths of gas and exposed to the gas for one hour. There was a check of 750 buds. The buds were cut from the Early Crawford, Late Crawford, Reeves Favorite, Elberta and Beers Smock varieties of trees in the nursery. The check buds were taken from the same varieties of trees.

An examination of the buds in the experiment October 3, 1906, more than a month after fumigation, showed an average of 98.5 per cent of the buds to be alive, while an examination and count of the buds August 30, 1907, showed 62.6 per cent of those exposed to the gas one half hour and 53.2 per cent of those exposed one hour to be alive. Only 20.9 per cent of the buds in the check were alive at the final examination. The normal per cent to grow in this state is from 60 to 65 per cent.

2. A second experiment conducted at another nursery September 1, 1906, consisted of 2,647 peach buds, Elberta and Champion varieties about equally divided into different tests. They were fumigated with 0.15, 0.18, 0.20 and 0.25 grams of cyanide per cubic foot, with 30 and 60-minute exposures.

An examination of these buds October 9, 1906, showed only 41.2 per cent as an average of buds alive fumigated with the various strengths gas and exposed for 30 minutes, while 49.1 per cent was the average of

those alive that were exposed to the gas 60 minutes. This very poor stand, due to excessive wet weather in the experiment as well as throughout the field, caused the abandonment of further observations. It may be said that it so happened that in each test the 60-minute exposure gave a larger per cent of live buds than the 30-minute exposure at this examination.

3. On September 7, 1906, 270 peach buds cut from orchard trees were fumigated with 0.285, 0.35 and 0.40 grams of cyanide per cubic foot, with an exposure of 30 minutes. An equal number of Elberta and Heaths Freestone varieties were employed in each test and all buds were known to be infested with San José scale.

An examination of the buds on October 13, 1906, showed 100 per cent of the buds in all tests to be alive, while at the final examination, August 30, 1907, an average of 34.2 per cent of the buds were alive. At this examination no live scale could be found on any of the buds, showing that all the strengths killed the scale.

4. On August 27, 1907, 624 peach buds, Late Crawford variety, were fumigated with 0.18, 0.20 and 0.30 grams of cyanide per cubic foot, exposed 45 minutes. All buds in the test were known to be infested with the San José scale. At the final examination, September 10, 1908, an average of 52.7 per cent of the buds in the test and 52.2 per cent of those in the check, which were not fumigated, were alive. No live scale could be found on any of the buds fumigated.

5. On December 4, 1906, 500 nursery trees were fumigated in ten different lots of 20 peach and 30 apple with 0.15, 0.20, 0.30, 0.40 and 0.50 grams cyanide per cubic foot, for 30 and 60 minutes respectively. All trees were badly infested with the San José scale.

At the final examination of the trees, September, 1907, all were living and vigorous. Living scale was found on some of the trees in each test below those fumigated with 0.30 grams per cubic foot, with an exposure of 60 minutes.

A second experiment, consisting of 8 tests with 10 apple trees in each, badly infested with scale, fumigated with 0.15, 0.20, 0.28 and 0.50 grams cyanide per cubic foot and exposed 30 and 60 minutes respectively, was conducted November 2, 1906. This lot of trees were delayed *en route* to College Park and accordingly were not in good condition for planting upon arrival. For this reason no accurate conclusions can be drawn, but at the final examination an average of six trees in each test were living and no more trees were dead in the test with the highest strength of gas and the 60-minute exposure than in the test of the weakest gas, with shortest duration of exposure. Further

living scale was found on some of the trees in each test below 0.20 grams per cubic foot with the 60-minute exposure.

Conclusions

The experiments in fumigating buds demonstrate that a much higher strength of gas and longer exposure than is ordinarily recommended, viz., 0.16 to 0.20 grams per cubic foot, can be employed without endangering the vitality of the buds, and that the exposure to the gas, even at the normal recommended strength, should be at least 45 minutes. In fumigating nursery trees at the normal recommended strength, viz., 1 ounce of cyanide to 100 cubic feet, the duration of exposure should be one hour, and if less time is desired the strength of the gas may be increased with perfect safety to the trees, in order to insure as far as possible the killing of any scale that may be present.

Dipping Experiments

Numerous inquiries as to the effectiveness and practicability of dipping nursery trees in lime-sulfur or other spray mixtures as a preventive against the dissemination of San José scale, either for the purpose of substituting such treatment for fumigation with hydrocyanic acid gas at the nursery or giving such additional treatment by the orchardists before planting as a further means of killing any scale that may have escaped previous treatment, led us to conduct a series of experiments in dipping during the last three years. The tests were not as extended as was desired on account of various circumstances, but the results will add to the rather limited amount of work published on the subject.

1. During March, 1906, 240 dormant nursery trees were dipped instantaneously in the lime-sulfur and salt wash, which was boiled for one hour with steam.

a. 40 apple and 20 peach were dipped, roots and stems in the mixture at a temperature of about 170 degrees Fahrenheit, and a like number were dipped, stems only, in the same mixture.

At the final examination, September, 1906, all trees in the first lot were dead. Only two had started to grow, and all the trees in the second showed that their stems had been scalded. 23 apple and 16 peach in this test had started at or near the top of the ground and were growing.

b. 40 apple and 20 peach were dipped, roots and stems, in the mixture, at a temperature of 120 degrees Fahrenheit, and a like number were dipped, stems only, in the mixture at the same temperature.

Of the first lot, only 10 apple and 3 peach were alive, and of the second, 30 apple and 19 peach were alive and doing well.

2. 69 apple and peach trees were dipped instantaneously, tops only, in the lime-sulfur wash, at a temperature of 122 degrees Fahrenheit, April 27, 1907. In addition about 100 well-grown plants of California privet were dipped in the same solution. Both the trees and privet were infested with the San José scale.

Examination November 29, 1907, showed no injury apparent and no scale could be found on any of the plants.

3. On November 29, 1907, about 200 peach and apple trees, 100 of which were infested with San José scale, were dipped in five different solutions, viz., home-made lime-sulfur at 100 degrees Fahrenheit, Scalecide, Target brand emulsion, Soluble Oil and Kiloscale, the oils being diluted 1 to 15. Part of the trees were dipped roots and stems, and stems alone, in each solution, also scaly trees were dipped in each wash.

Examination of the trees during the past summer and finally October 16, 1908, showed no trees to have been injured in any way by any of the solutions and further no scale could be found on any of the trees.

4. The above experiment was duplicated as far as possible this past spring, the trees being dipped in the same solutions, except that San-U-Zay was substituted for Kiloscale, April 14 and 16, 1908.

Examination of these trees during the past summer and finally October 15, 1908, showed more or less injury by the various oil solutions, but this is no doubt due to the fact that all the trees used in the experiment were just beginning to leaf at the time of dipping. All the trees dipped in the lime-sulfur wash lived and seemed not to be hurt by the solution, while all the trees in two tests dipped in San-U-Zay died. All the trees in the check lived and were thrifty. Further, no scale could be found on any of the trees that were infested before dipping at the final examination.

Conclusions

The experiments of 1906 showed that dipping dormant nursery trees, either roots and stems, or stems alone, in the lime-sulfur wash, at a temperature from 122 degrees to 172 degrees Fahrenheit, may be expected to be attended by severe injury to the trees.

In the 1907 and 1908 experiments no difference could be detected from the appearance of the trees dipped, roots and stems, or stems only, in the lime-sulfur, at a temperature of 100 degrees Fahrenheit and in the various oil mixtures at the strength of 1 to 15. However,

we believe that the dipping of the roots of trees in any of the mixtures at any time to be a rather questionable procedure. They also show that only strictly dormant nursery stock should be dipped in any of the solutions used.

While no scale developed on any of the infested trees after the treatment in either experiment, in view of the published results of other experimenters and the limited number of infested trees included in these tests, we consider that these results may be inconclusive.

Finally these varying results go to show that further experimentation is necessary in order to establish what may be expected from such treatment, even if its adoption for general use should become desirable.

PRESIDENT FORBES: Any discussion on this paper?

A MEMBER: I would like to ask whether you would recommend dipping to the grower who wants to take every precaution to have his trees free from scale? I had that question from a correspondent the other day and would like information concerning it. We get scale right along on our trees when the certificate says they have been fumigated. With the average man fumigation is a little difficult. Is fumigation safe enough and satisfactory enough to warrant our advising growers to dip the nursery stock, omitting dipping the roots?

MR. PARROTT: We have made some experiments in dipping trees. If the farmer desires to dip, I would suggest that he use one of the miscible oils. I do not believe that the lime-sulfur wash has the penetrating qualities of the miscible oils, for in our comparative tests of these sprays we have had much better results with the latter. Moreover, I do not believe that the average farmer will fumigate, and if he desires to treat his trees before planting, I would suggest a miscible oil, but I think it would be better for him to plant his trees first and then to spray them with either the lime-sulfur wash or a miscible oil.

MR. SKINNER: Mr. President, I don't know that it has ever been tried in dipping, but I would like to have some one try solutions of potassium cyanide of varying strength. It ought to be very effective and very cleanly and inexpensive.

MR. J. B. SMITH: I would like to answer Doctor Skinner that it is extremely effective. It kills every plant dipped in it.

MR. SKINNER: I would like to ask Doctor Smith the strength?

MR. J. B. SMITH: I run it down to where it wouldn't hurt insects and it still killed every plant I tried it on.

MR. PARROTT: Since this question has come up, I would like to ask if any of the members have tried any of the concentrated tobacco pre-

parations for the woolly aphis. I have always thought there might be some field of usefulness for these preparations, as dips for this pest.

A MEMBER: Mr. President, last year part of our trees were dipped in a tobacco preparation for the woolly aphis and for the green aphis on the tops. We expect to dip about 20,000 this year. I hope that Mr. Parrott will try it. We are not certain how effective it is.

MR. HITCHINGS: Last week a gentleman came to me and asked if he would be allowed to bring scions from an infested district to Maine, as he wished to use them to graft his trees. He wanted to secure several thousand. Would it be safe to have these scions dipped or should they be fumigated?

A MEMBER: Fumigated every time, I should say.

[The remainder of the Proceedings will appear in the next issue.—ED.]

THE LIFE HISTORY OF THE ARGENTINE ANT

Iridomyrmex humilis Mayr

By WILMON NEWELL.

A general account of this species and its habits appeared in the JOURNAL OF ECONOMIC ENTOMOLOGY, Vol. I, p. 21 to 34 and accounts of two inquilines occurring in its nests were given in the same volume, p. 262. On p. 289–293, Mr. E. Foster published an interesting account of the introduction of this ant into New Orleans.

The present paper is intended to deal only with the more salient features of this insect's life history which have been brought to light in the course of the author's studies during the past two years.

There are but three adult forms in the case of this ant, the queen, male and worker. Of the immature forms there are three, egg, larva and pupa, of each the queen, male and worker. There is hardly sufficient difference between the virgin queen and the deälated queen after fertilization to justify considering them as distinct forms. A possible fourth stage may be recognized in the "callow," which is the term used by some writers in referring to the worker which has completed its transformation from pupa to adult but which has not attained the normal worker color and activity. A complete colony may therefore consist of queen and workers only, of queens and workers or of queen (or queens), males and workers: with each of these combinations may be associated any one or all of the three immature stages corresponding to each of the three adult forms. Of

nine immature stages in all. Plate 5 shows a colony consisting of one queen, about 100 workers and about 20 eggs, with no larvæ, pupæ or males present.

In size the colonies may vary from a dozen to many thousands of individuals and the number of queens present in a colony may vary from one to many hundreds. Though the Argentine ant is particularly aggressive and a hard fighter when coming in contact with most other species of ants, there is no apparent antagonism between separate colonies of its own kind. In fact, in heavily infested areas the workers and queens are so intermingled that the individuality of colonies is entirely lost sight of and all colonies appear to become part and parcel of one enormous "community." In this respect the species may be said to have a more perfect social organization than even the honeybees, colonies of which are very distinct and the individuals of which repel with alacrity any visitor from another colony.

Methods of Study

When the study of this ant was undertaken two requisites presented themselves, a form of artificial formicary in which continuous observations could be made and individuals kept track of from the time of egg deposition until the adult stage was reached, and some method by which all individuals of a colony could be confined to their own formicary.

Artificial formicaries, or cages, of various types were made and tried. Among them were cages consisting of two glass cylinders placed one within the other, the intervening space filled with soil, the Janet cage, molded of plaster of Paris and having several compartments, and wooden and glass cages constructed in the form of cubes, from which the ants could not escape. None of these met the requirements. In the cylindrical cages crumbling earth often destroyed the galleries and it was impossible to so regulate the space between cylinders that the ants could not construct invisible galleries into which eggs and larvæ were carried.

The Janet cages proved successful only in the case of very large colonies, but in these the multiplicity of individuals made accurate observations impossible. It may be remarked that this type of cage is excellent for studying the community life as a whole and for making experiments with poisons or with parasitic fungi or bacteria.

Cages totally enclosed were not successful for the reason that the ants, when deprived of the privilege of leaving their nest, failed to act in a normal manner.

The cage finally adopted was, with modifications, the one described

by Sir John Lubbock on pages 2 and 3 of his classic work.[1] This consists essentially of two glass plates, containing between them a layer of pulverized earth in which the ants may burrow at their pleasure. Considerable difficulty was experienced in getting the glass plates the proper distance apart: if too for apart the ants could make burrows which were not open to observation and if too close together insufficient room was afforded the queen in which to stand and walk upright. As the queen is about twice as tall as the worker it seemed for a time that a suitable cage could not be constructed.

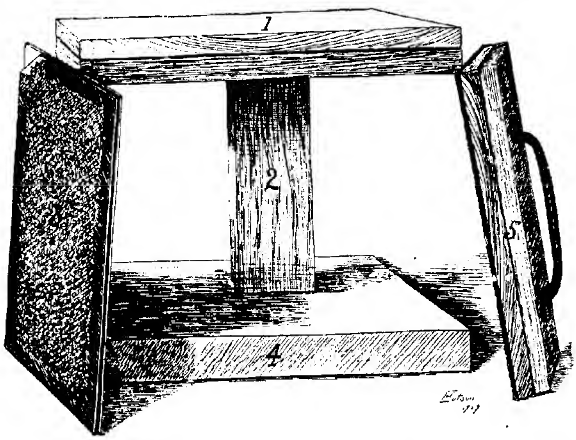

Fig. 1.—Artificial formicary or cage used in studying the Argentine ant: 1, supporting platform; 2, standard; 3, cage proper, made of glass and leather, containing earth; 4, base; 5, cover.

After repeated trials, however, it was found that if the space between the glass plates were made exactly 1.75 mm. the queen would have sufficient room and the workers could not construct invisible galleries.

This type of cage and its supporting stand are well illustrated by Figures 1 and 2. Figure 1 shows the several parts of the cage; "3" is the cage proper, consisting of two plates of glass held uniformly 1.75 mm. apart by strips of leather at all four edges, a door or opening being left at one corner (See Figure 3). Old negatives, the films removed with caustic soda, have been found the most desirable for making these cages, both because such glass is remarkably clear and free from imperfections and because it is of uniform thickness. The

[1]Avebury.—"Ants, Bees and Wasps," 1881.

size of the cage may vary from 3¼ x 4¼ up to 8 x 10 inches or even larger. Leather was found more satisfactory for making the edges of the cage than either glass or wood. The strip of leather between the glass margins is about ½-inch in width. It is extremely difficult to find a strip of glass uniformly 1.75 mm. thick and it is also difficult to firmly attach one piece of glass to another. Wooden strips present the disadvantage of quickly decaying and of warping, no matter what glue or cement is used to hold them in position. Since

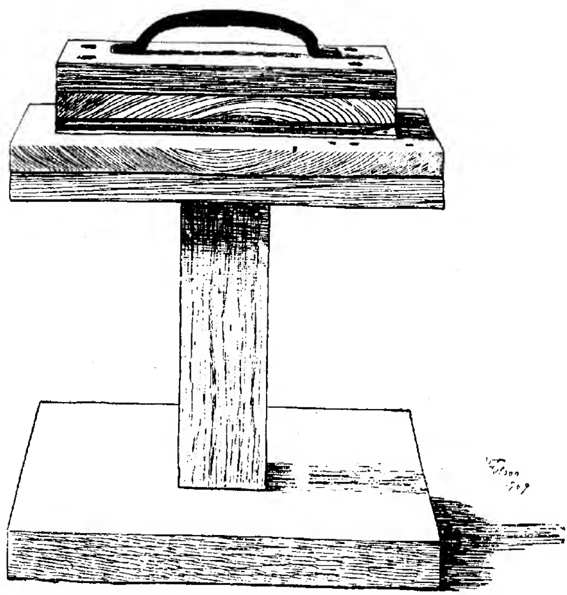

Fig. 2.—Artificial formicary with parts assembled ready for use.

it is sometimes desirable to place moist earth in the cages, or to add moisture from time to time, a waterproof cement is most desirable for attaching the glass plates to the leather strip. For this purpose the cement known as coaguline has been found satisfactory. The space between the glass plates is filled with finely pulverized earth, after completion and drying of the cage, and in this the ants are permitted to burrow and construct galleries as they please.

The cage proper is supported on a platform (1) which in turn rests firmly upon a standard (2) having a base (4). The platform must have its upper surface perfectly level and it must remain so for an

indefinite time, otherwise the ants will take up their abode between the cage and platform rather than in the cage itself. The platform is therefore made of two pieces of even, seasoned cypress ⅞ inches thick, screwed together with numerous screws and with the grain of the two pieces at right angles to each other. On this platform the cage rests without fastenings of any kind. The cover (5) is constructed of two pieces of cypress in the same manner as the platform, but in addition has an iron handle attached to its upper surface and has a piece of felt glued to its under surface so that, when it is placed upon the cage proper, all light is excluded except at the entrance. The cover is of the same outside dimensions as the cage itself. To insure the platform remaining level it is often necessary to make

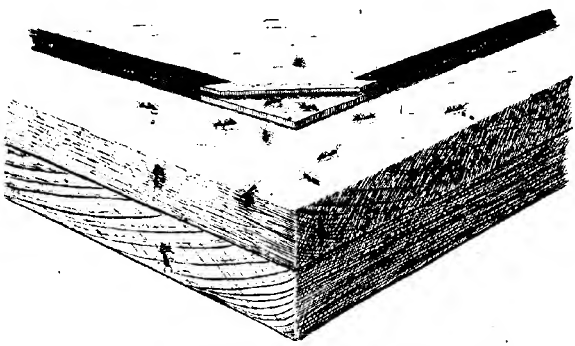

Fig. 3.—Entrance of formicary shown in figures 1 and 2 (figs. 1, 2 and 3 from original drawings by Miss Ethel Hutson).

the base of two pieces in the same manner as the platform, or to nail strips across it at right angles to the grain. Both platform and base are attached to the standard by long screws with heads countersunk. Food is furnished by placing it on a piece of cardboard at any point on the cover or platform. The base stands in running water, as explained below. This type of cage permits the ants to leave their nest within the cage and to forage over the platform, cover and stand in natural fashion, but their escape from the stand is prevented by the very natural barrier of water which they find when they approach the bottom of the standard. It is not possible for them to conceal larvæ or eggs where the observer cannot find them and they cannot bring in larvæ or pupæ from outside sources, to the annoyance and vexation of the student.

While the ants are very fond of sweets we have found that sweets

alone will not suffice for food indefinitely. Animal food is also required and we find that by supplying the colonies with a "balanced ration" of honey and fresh beef or veal, they will work in a perfectly natural manner for many months without other food.

The problem of confining the ants to the cage and its stand was not so easily solved. We first tried Sir John Lubbock's method of placing a moat of glycerine or water about the stand, but both liquids dried too quickly and were effective for only a few hours. Recourse was had to the proverbial chalk line without success. Bands or ditches of kerosene, crude oil, tar, oils of sassafras and citronella, tree tanglefoot, zenoleum, naphthaline, coal tar disinfectants, whale-oil soap, sharp-edged tin and fur were all failures. Certain powerful odors, such as those of zenoleum, sassafras and citronella, act as repellents temporarily, but after a few hours of evaporation are no longer effective. Ordinarily these ants will not cross bands of cotton tape which have been impregnated with a saturated solution of corrosive sublimate and dried, but when attempting to leave an area to which they have been confined by this means they are much more persistent in crossing it.

Water with a film of whale-oil soap on it acted as a repellent for a few hours only, while a film of kerosene upon water merely afforded a convenient floor upon which the ants could travel. The difficulty in confining the workers with any liquid or mucilaginous substance lies in the fact that they are exceedingly light[2] and sticky substances shortly harden on the surface so that the workers are supported. The surface film of clear water is in fact almost strong enough to support a worker not loaded. It is not unusual to see an ant alternately walking and swimming in crossing a narrow ditch of water which has been standing for a few hours. Minute dust particles collecting upon standing water shortly form a film upon which the workers pass with ease. Perfectly fresh water therefore served to confine the colonies to their cages and at first our observations were made upon colonies in cages which were standing in dishes of water. This, however, necessitated frequent changing of the water, and observations were often brought to an abrupt finish by other duties preventing the change of water in the vessels at the right time.

Our next step was to construct a small building, 10 x 30 feet, equipped with benches having upon them galvanized iron trays 2½ x 12 feet, 4 inches deep. In these trays the cages are placed and by suitable connections running water 2 inches deep is kept

[2]The average weight of one worker is 0.0002077 grams.

passing through the trays day and night. As the ants will not voluntarily enter running water this method has worked admirably. The building in which this work is carried on is shown in Plate 6. The iron trays and ant cages are shown upon the right, with work tables, chemicals, etc., on the left. The building is equipped with electric lights and extension lights for night examination, in addition to gas, and a combined hygrograph and thermograph records the temperature and humidity of the room at all times. For convenience we have called this special building a "formicarium"—which the office boy invariably confuses with "auditorium" and "natatorium." Plenty of windows insure full ventilation at all seasons; and to avoid abnormally high temperatures in summer a second or accessory roof, 2 feet above the main roof, breaks the rays of the sun and shades the building proper. The building has also proven a convenient insectary for the breeding of other insects. The Argentine ant possesses a marked proclivity for attacking all insects which one has *under observation*, and all breeding experiments in cages, no matter what the insect, must be protected from the ants. The trays of running water therefore serve to keep the ants away from general cage experiments, as well as to confine the ants to the cages in which they themselves are being studied.

It may be mentioned that Prof. C. W. Woodworth of California visited this "formicarium" in the summer of 1908 and so pleased was he with the cages and the plumbing arrangements of the formicarium that he returned to California and prepared a similar outfit for the study of the Argentine ant there.

Establishing Colonies for Study

To establish a colony in one of the artificial formicaries or cages is comparatively easy. It is only necessary to secure a fertile queen from some thriving outdoor colony and place her on the stand, first placed in water, together with any desired number of workers which have been captured by attracting them to a sweetened sponge or piece of fresh meat. Any lot of workers will accept any queen and *vice versa*. When queen and workers are thus placed upon the cage and its stand, they usually, after a few hours, take up their abode in the nest proper. At first we experienced some difficulty in preventing them from collecting beneath the stand, but it was presently found that if a little dirt be removed from another colony and placed in the entrance of the new formicary the ants would enter at once and adopt it as a suitable home. After the establishment of such colonies the queen usually commences egg deposition in from 6 to 48 hours.

By establishing colonies in this manner, without immature stages present, it is easy to observe the daily rate of egg deposition, the incubation period of the eggs, and the duration of the larval and pupal stages. In some of the records given below single individuals have been kept under observation from deposition of the egg, through larval and pupal stages, to the adult. In other cases the time from deposition of the first egg until hatching of the first larva was assumed to be the period of incubation, date of hatching of first larva to formation of first pupa the duration of larval period, etc.

The Queen

The deälated queen is well illustrated at e, figure 4.[3] The deälated queen measures from 4.5 to 5 mm. in length and queens measuring 6 mm. in length are not uncommon. It should be remarked here that during egg laying periods the abdomen is much larger and longer than shown in the drawing. Normally the abdomen extends well beyond the tarsi of the hind legs. Unfortunately, a drawing cannot show the delicate silky pubescence of the queen's body and in life she is a far more beautiful creature than one would imagine from the drawing, correct though the latter is in anatomical detail.[4]

The credit for first discovering and recognizing the queens of this species seems to belong to Mr. E. Baker, formerly superintendent of Audubon Park, New Orleans, and Prof. R. E. Blouin, formerly in charge of the Audubon Park Experiment Station. Queens found by them in August, 1905, are still in the writer's collection.[5] If Mr. Titus was familiar with the queens he evidently failed to mention it in his interesting account of this species.[6]

The rate at which the queen deposits eggs varies with the prevailing temperature and egg deposition is suspended entirely at low temperatures. In the artificial formicaries, already described, the num-

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[3]The writer is under obligations to Dr. W. M. Wheeler for a critical examination of the drawings shown in figure 4 prior to their engravure.

[4]For a detailed description of the queen, see JOURNAL OF ECONOMIC ENTOMOLOGY, I, p. 29.

[5]Following is the letter from Professor Blouin, announcing the finding of these queens:

Audubon Park, New Orleans, La.,
August 21, 1905.

Mr. Wilmon Newell, Shreveport, La.

DEAR SIR: I enclose you a few specimens of the queen ant of the species recently investigated here by Mr. Titus, named by him the New Orleans ant, or *Iridomyrmex humilis* Mayr. These were collected by Mr. E. Baker, Supt. of Audubon Park, in his nursery right close to us.

(Signed) R. E. BLOUIN.

[6]Bulletin 52, Bur. of Entomology, p. 79.

ber of eggs laid each day varies from one or two to as many as fifty or sixty. Thirty per day is not far from the normal number in warm weather when the food supply is abundant. It appears probable, however, that the queens deposit much more rapidly in large colonies, although, from the nature of the case, this cannot be verified by direct observation. Egg deposition becomes very slow, or ceases entirely, when the daily mean temperature falls below 68° F.

Practically all queens under observation have shown a disposition to suspend egg deposition entirely for longer or shorter periods.

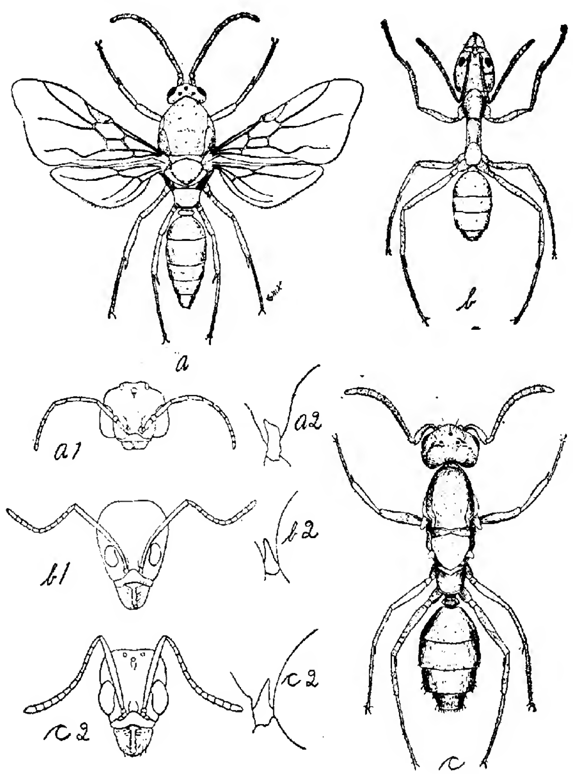


Fig. 4.—The Argentine ant: *a*, adult male; *a1*, head of male; *a2*, petiole of male; *b*, worker; *b1*, head of worker; *b2*, petiole of worker; *c*, fertile queen; *c2*, head and petiole of queen; all greatly enlarged (from original drawings made under the author's direction by Miss C. M. King).

even when such "off" periods cannot be accounted for by low temperatures.

Fertile queens confined in test tubes without accompanying workers will often deposit a few eggs upon the walls of the tubes, but we have been totally unable to get colonies established by confining queens in artificial formicaries without workers accompanying them. This failure has not been due to any need of workers to feed or care for the queen, since she can feed herself from a supply of honey or sugar as readily as can a worker. Ordinarily, she attends to her own toilet and it is doubtful whether she is in reality "attended" by the workers in the sense that queen bees are attended.

Fertile queens do not confine themselves to the formicaries, either natural or artificial. Isolated deälated queens are not infrequently found wandering about buildings by themselves and, while the queens in artificial formicaries ordinarily stay within the nest proper, they have at times been seen outside of it. The finding of deälated queens wandering about, coupled with the fact that workers readily accept a queen from any source, seems to indicate that new colonies may sometimes be established in nature by workers associating with such wandering queens. This probable method of colony formation is quite distinct from the pronounced division, or "divisional migration," which is referred to on a subsequent page.

All immature stages of the queen are as yet unknown. In the two seasons during which colonies have been under almost daily observation, not a single queen has been developed in any of the artificial formicaries, though males have been developed in abundance.

Virgin queens should be easy to discover in areas heavily infested by the species but such is not the case; nor have we been able to detect any of the virgin queens when the males were flying in abundance. Only three virgin queens have thus far come under our observation. These were found on April 21, 1908, by Messrs. Rosenfeld and Barber, two of my assistants, in the course of examining a very large colony which had been driven from a low marshy area by the rising waters of the Mississippi River.

The Worker

The worker measures from 2.25 to 2.75 mm. in length and is well illustrated in figure 4, *b*. As with the queen, the abdomen extends to about the tarsi of the hind legs when the worker is active or engaged in feeding. The abdomen is capable of considerable distension, and when the worker is fully engorged with syrup or other liquid the chitinous plates of the abdomen are forced apart, rendering

the connecting membranes distinctly visible. The writer has often noticed workers returning from their attendance upon plant lice with abdomens so distended that they looked like little drops of silvery liquid. Particularly is this appearance presented when the returning workers are viewed with a strong light beyond them.

As already stated there is but one caste among the workers. In a large colony there seems to be something of a division of labor, certain ones engaging in foraging, others in nursing and still others in excavating or sanitary work. However, any individual worker can assume the duties of any other and does do so when exigencies demand. Worker callows, barely hardened into mature adults, go forth in search of food and the hardened veterans of many months' service seem to make as efficient nurses as even the youngest.

The workers are particularly long-lived. A colony of about seventy workers was made queenless and broodless on July 8, 1908. By October 10th the number of workers had become reduced to about forty and some of the original ones survived until February 25, 1909, a period of 6½ months. But for the fact that many of these workers met death accidentally a longer period of survival would doubtless have been recorded.

The Egg

The egg, which is to produce a worker, is elliptical, about .2 mm. wide by .3 mm. long. It is pearly white, lustrous and without markings (See Plate 7, A). As time for hatching approaches it loses its brilliancy and the surface takes on a duller appearance. This is not sufficiently pronounced and uniform, however, to be taken as a safe guide to immediate hatching. The egg membrane is exceedingly thin, so thin in fact that when the embryo has taken on the larval shape, the membrane not infrequently adapts itself in a way to the general contour of the enclosed embryo, thus making it very difficult to distinguish between eggs and just-hatched larvæ.

Some care of the egg by the workers seems essential to complete embryonic development. Eggs deposited in test tubes by isolated queens have gone through a portion of the embryonic development, but we have not been successful in getting them to hatch. This may be due in part to the ease with which the delicate embryos can be injured in handling and to the fact that when placed on glass condensing moisture may retard or stop development.

Incubation

The eggs, after deposition by the queen, hatch in from 18 to 55 days, according to the prevailing temperature. The longer periods

are doubtless accounted for by embryonic development being entirely suspended during cool weather, and it is not impossible that the viability of eggs may be entirely destroyed by a temperature as low as 25 or 30°, but on this point we are as yet undecided.

The period of incubation has been determined, ordinarily, by placing a queen and workers, but no immature stages, in an artificial formicary and then noting the time from deposition of the first egg to appearance of the first larva. This period was assumed to be the real period required for incubation. In other cases, single groups of eggs have been kept under constant observation throughout the entire period of incubation. The following table shows the variation in development at different seasons, together with the average daily mean temperatures prevailing:

TABLE I

DURATION OF EGG STAGE AT DIFFERENT SEASONS—WORKER

Record No.	From	To	Days.	Average daily mean temperature during period.	Average daily mean humidity.
1......	Oct. 1, 1907	Nov. 15, 1907	45½	*	....
3.......	Dec. 22, 1907	Feb. 14, 1908	55	*	....
4.......	Mar. 14, 1908	April 9, 1908	27	70.3°F.	70.2%
6.......	May 1, 1908	May 23, 1908	23	74	68.9
7.......	July 20, 1908	Aug. 10, 1908	22	81	82.9
8.......	July 25, 1908	Aug. 12, 1908	19	81	81.5
		Average	32		

The Larva

The larva when first hatched is not distinguishable from the egg without the assistance of a magnifying glass. For a time after hatching the body is severely curved, the cephalic end being almost in touch with the caudal end, but as development progresses the larva assumes more and more of a straight form. The curvature is not entirely lost, however. A recently hatched larva, measured with the compound microscope and eye-piece micrometer, measured .49 mm. long by .32 wide. The fully grown larvæ (workers) average 1.7 mm. long by .66 mm. wide. The largest one under our observation measured 1.87 mm. by .765 mm.

*Cages kept in office; record of exact temperatures not available. The balance of the records were made in the "formicarium" and the recording instruments kept in the same room with the cages, hence the temperature and humidity records are correct for the exact location of the eggs under observation.

The larvæ are fed often by the attending workers upon regurgitated, and presumably predigested, food. There is nothing in the appearance or actions of the workers which do the feeding to indicate that they are different from those which perform other duties, or that they are assigned to the particular and exclusive duty of being nurses. The feeding of the larvæ has several times been observed under a magnifying glass and is as follows: The larva ordinarily lies upon its side or back. The attending worker approaches from any convenient direction, usually from one side or from the direction in which the head of the larva lies and, spreading her mandibles, places them over the mouth-parts of the larva which are slightly extruded. The tongue of the worker is also in contact with the larval mouth. While the worker holds body and mandibles stationary a drop of light-colored, almost transparent fluid appears upon her tongue. This fluid disappears within the mouth of the larva, but it cannot be ascertained to what extent the larval mouth-parts are moved during the operation, owing to their being obscured from view by the mandibles and head of the attending worker. Slight constrictions of the larval abdomen during feeding are sometimes noticeable, at other times not. The time required for feeding a single larva varies from 3 to 30 seconds, depending doubtless on the hunger of the "baby." The workers proffer food to, or at least inspect, each larva, for the worker doing the feeding will place her mandibles to the mouth of one larva after another, feeding those which seem to require it.

Both larvæ and pupæ are groomed or licked with the tongues of the workers; thus are they ever kept in a state of absolute cleanliness.

The most pronounced increase in size of the larvæ occurs during the first five days after hatching; after that it is relatively slower. As the larva increases in size the contents of the alimentary canal, dark in color, can be seen through the walls of the abdomen.

Just prior to transformation into pupa, the larva takes on a rather characteristic appearance, which if it were more distinct, would justify characterization as the "pre-pupal" stage. In this stage the cephalic and thoracic portions of the larva become markedly smooth and shining, with segmentation indistinct or absent. At the same time the line of demarcation between thorax and abdomen becomes more distinct and the contents of the alimentary canal appear to be shifted nearer to the caudal end than in the larva proper. The mouth-parts, indistinct in the larval stage, now appear more prominent. The difference between larval and pre-pupal stages is by no means pronounced, but with practice one can predict the approaching transformation to pupal stage by it with reasonable accuracy.

The duration of the larval period has been determined by observation in the artificial nests in the same manner as the incubation period, already described.

The following table shows the duration of the larval period at different seasons:

TABLE II

DURATION OF LARVAL STAGE AT DIFFERENT SEASONS—WORKER

Record No.	From	To	Days.	Average daily mean temperature during period.[1]	Average daily mean humidity.
1........	Nov. 16, 1907	Jan. 15, 1908	61	52.2°F.	....
6........	Feb. 5, 1908	April 1, 1908	57	62.2	71.9%
8........	Feb. 5, 1908	April 1, 1908	57	62.2	71.9
10........	Feb. 15, 1908	Mar. 28, 1908	43	62	72
3........	Feb. 29, 1908	Mar. 26, 1908	27	67	73
9........	April 10, 1908	April 24, 1908	15	76.6	75.8
7........	April 12, 1908	April 25, 1908	14	76.1	75.2
2........	July 19, 1908	Aug. 1, 1908	14	80.5	82
11........	Aug. 13, 1908	Aug. 27, 1908	15	81.7	71.7
4........	Sept. 4, 1908	Sept. 14, 1908	11	81.1	73.6
		Average	31		

The Pupa

The pupa immediately after transformation from the larval stage is pure white, without markings, except that the compound eyes are prominent as jet black spots upon the head. The pupa is slightly larger than the grown larva, the average length being about 2 mm. The head is by far the most prominent portion. A pupa measuring 2.04 mm. in length was found to have a head 1.19 mm. in length (dorso-ventral diameter) while the thorax and abdomen measured .51 and .561 mm. respectively. The pupæ are shown in Plate 7, *b* and *d*.

As time for transformation to adult approaches the pupa changes to a creamy color, then through a light brown to a dark brown, the latter shade being practically identical with the body color of mature workers. The time of these changes varies with the duration of the pupal stage, but the following record of changes in color of a pupa which occupied a full twenty days from larva to adult (callow), is near the average:

[1]We have not attempted to give the accumulated effective temperature necessary for the development of different stages, as we are not satisfied as to the critical point from which it should be computed. It is doubtless higher than 43° F.

1st to 17th day—Pupa pure white, except compound eyes.
18th day—Turned to a light creamy yellow.
19th day—Became a light brown.
20th day—The brown color deepened.
21st day—Reached teneral stage.

In some colonies there is more or less of an indistinct sorting of the immature stages, pupæ being placed in one portion of the nest and larvæ in another. This tendency is not perceptible in many colonies and is usually most noticeable in very large colonies.

The duration of the pupal stage has been determined in the manner already described for the incubation and larval periods. The range of pupal development is shown in the following table:

TABLE III

Duration of Pupal State, Individual Workers, 1908

Record No.	From	To	Days.	Average daily mean temperature during period.	Average daily mean humidity.
1........	Jan. 21	Feb. 14	25	58.5° F.	68.3%
2........	Mar. 14	Mar. 27	14	67.5	71.8
5........	Mar. 26	Apr. 11	17	73.8	68.9
6........	Mar. 30	Apr. 14	16	73.8	70.2
3........	Apr. 5	Apr. 15	11	76	73.5
10.......	Apr. 5	Apr. 18	14	76.3	74
7........	Apr. 5	Apr. 20	16	76.7	74
8........	Apr. 8	Apr. 23	16	76.6	74.5
9........	Apr. 25	May 13	19	71	63.5
11.......	Apr. 25	May 14	20	71.2	61.4
4........	Aug. 1	Aug. 11	11	82.2	80
12.......	Aug. 6	Aug. 16	11	83	74.8
13.......	Aug. 10	Aug. 20	11	82.8	70.7
14.......	Aug. 28	Sept. 7	11	81.4	71
		Average	15		

The Callow or Teneral Stage

During the last few hours of the pupal stage the legs, mouth-parts and antennæ become more prominent and the pupa is assisted in its transformation by the workers, who attempt to straighten out the legs and antennæ. We are convinced that there is a very thin transparent membrane or skin surrounding the pupa, which is shed at time of transformation but its existence is difficult to establish satisfactorily.

Immediately after transformation the young worker is colorless, almost transparent, but is otherwise identical in appearance with fully matured workers. To this stage, following the custom of some authors, we apply the term "callow." The callow is at first very clumsy and walks with uncertain steps and staggering gait, reminding one much of a worker bee just emerged from the brood comb. During this stage the workers seem still to feel a responsibility for the callow's welfare, for upon the colony being disturbed the callows, like larvæ and pupæ, are unceremoniously grabbed up by the workers and hustled to a place of safety.

The body of the callow deepens in color quite rapidly and in from 48 to 72 hours after transformation from the pupa becomes indistinguishable from other adult workers.

Time Required for Complete Development

By adding together the minimum periods required for the development of eggs, larvæ and pupæ, as given in Tables I, II and III, we find that at least 41 days are required for development from egg to adult and in a similar manner addition of the maximum periods gives 141 days as the maximum time required.

From the tables also it is seen that the average period of incubation for the eggs is 32 days, for development of the larvæ 31 days and for maturing and transformation of pupa to adult 15 days. By adding together these averages we arrive at 78 days as the average period of development. This of course cannot be termed the time required for the development of a generation, since workers do not reproduce and the term "generation" can be used only in referring to the succession of queens.

The Male

The appearance of the adult male is well illustrated in figure 4, *a*. The males average about 2.8 to 3 mm. in length. The most noticeable feature about them is the manner in which the thorax is enormously developed. The abdomen is relatively small and the head short and blunt. The shape of the head alone permits distinction between the male and virgin (winged) queen without the aid of a glass.

The normal time of appearance of the males in the colonies is in spring, but the appearance of a relatively small number in autumn is not uncommon. During mid-winter and mid-summer none are found. The males have been bred in the artificial formicaries in large numbers, hence abundant opportunity has been afforded to study their appearance and habits. The following account of their appearance in

one colony under observation is typical: In Cage 1 the first male pupa made its appearance on April 11, 1908, and by April 15 the male pupæ were numerous. The first of these reached maturity on May 1st. By May 11th the adult males in the colony numbered 11 and on this date some of them essayed a flight. For several days following daily flights were made, most of them terminating ignominiously in the galvanized iron trays of water.

On May 14th the male pupæ in this colony were still appearing abundantly and they continued to appear, at a deceasing rate, until June 27th. By July 8th but one male remained in the colony and this one disappeared by July 22d.

Examinations of the outdoor colonies during May showed males present in practically all of them. Flights out of doors were common during May but we were unable to find any virgin queens among the flying males.

In the autumn males are found in but a small percentage of the outdoor colonies and they rarely appear in the artificial formicaries. In December of 1907 one of my assistants found males exceedingly abundant in a single colony, while in another colony an assistant noticed them constantly present during all of November and December of the same year. In October of 1908 a few males were found in a single outdoor colony. We have found no virgin queens in the autumn.

The eggs which produce males are indistinguishable from those which produce workers and we have found no way to separate the male-producing larvæ from the worker-producing larvæ until just prior to pupation. The male larvæ grow to a somewhat larger size, on the average, than do the worker larvæ and it is thus possible to predict with some degree of certainty which of grown larvæ will transform to males and which to workers. As soon as transformation to pupa takes place there is no further confusion. The male pupa is fully 50% larger than the worker pupa and has, by comparison, an enormous thorax. The male pupæ vary in length from 2.78 to 3.23 mm., with an average length of 3.04 mm.[1] As the average length of the thorax alone is 1.9 mm., it is at once seen what a relatively large part of the body it constitutes. The male pupa is shown in the center of Plate 7.

When first transformed from the larval stage the male pupa is pure white, with exception of the compound eyes, which are faintly tinged with brown. Gradually the color of the compound eyes deepens and

[1]From measurements of 10 specimens by Mr. Arthur H. Rosenfeld.

the ocelli become visible as minute dark spots upon the head. The male pupa, like the worker pupa, passes through gradations of creamy yellow, light brown and dark brown to almost black before transforming to the adult stage. The color reached by the male pupa just prior to transformation is much deeper than that attained by worker pupæ. The males are assisted in their transformation to the adult stage by the workers, and the pupal skin, or at least a portion of it, is worked backwards to the tip of the abdomen and there shed entirely. Within a few hours after transformation the wings of the male become fully expanded. The following table shows the duration of the male pupal stage at different seasons:

TABLE IV

DURATION OF PUPAL STATE, INDIVIDUAL MALES, 1908

Record No.	From	To	Days.	Average daily mean temperature during period.	Average daily mean humidity.
1........	April 11	May 1	19¼	73.6°	69.8%
2........	April 14	May 4	20½	73.6	68.6
3........	April 14	May 4	20½	73.6	68.6
4........	April 17	May 10	24	72.8	67.3
7........	April 17	May 10	24	72.8	67.3
8........	April 18	May 11	24	72.2	66.7
9........	April 18	May 13	23	72.8	66.5
5........	April 20	May 13	24	71.8	65.8
6........	Sept. 24	Oct. 21	28	70.5	67.6
		Average	23¼		

Formation of New Colonies

Reference has already been made to the possibility of new colonies being formed by workers associating themselves with wandering or migratory (?) fertilized queens. However, the more common method of colony formation is a very different process. In the autumn months there is a marked tendency for colonies to unite and seek dry sheltered situations, such as masses of leaves, straw, etc., in which to pass the winter. Comparatively few colonies attempt to pass the winter in underground nests unless these are situated in protected places, as under buildings, boards, vegetation, etc. The large winter colonies frequently contain hundreds of queens. With the approach of warm weather small colonies, varying in size from one queen and a dozen or two workers to a half dozen queens and several hundred workers, migrate out from the large over-wintering colonies and establish them-

selves in new situations, by preference in soft earth. This spring movement is sufficiently pronounced to be termed a migration, and from its nature it seems best to call it a "divisional migration." In the spring of 1908 it was noticed to occur in the early part of March. The present season this movement began as early as February 13th. This spring division of large colonies into small also explains why large colonies are the rule in autumn and small colonies the rule in spring.

Scientific Notes

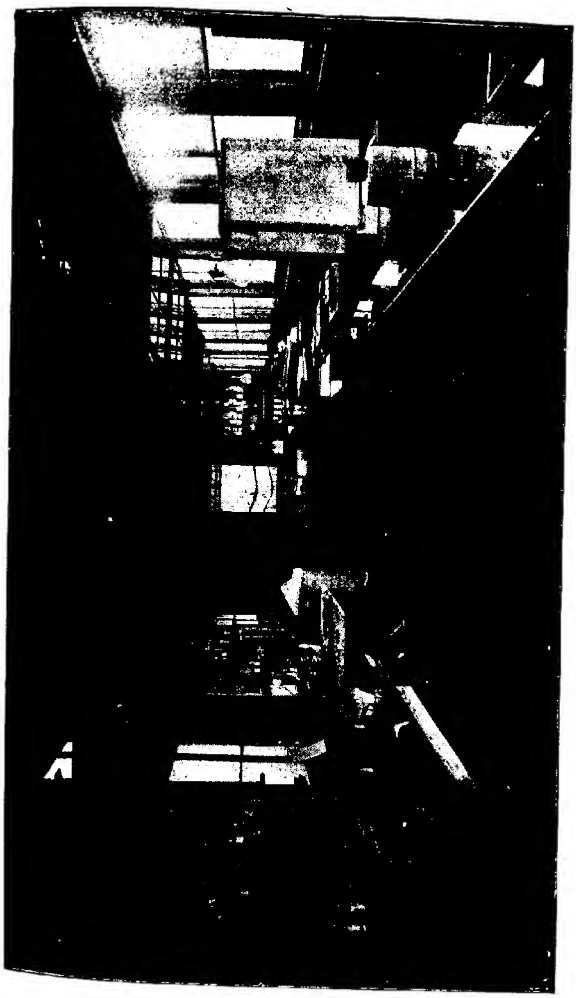
A Remedy for House Fleas.—In the latter part of last May (1908) I moved into a new house that had not been previously occupied. No carpet was used and being summer only a few rugs were placed on the floors. A part of the household consisted of a collie dog and three Persian cats. Very soon the fleas appeared, the dog and cat flea, *Ctenocephalus canis*. I did not count them and I can't say whether they numbered a million or only a hundred thousand. On arising in the morning and stepping on the floor one would find from three to a dozen on the ankles. The usual remedies for fleas are either drastic or somewhat unsatisfactory. The drastic one is to send the animals to the institutions, where they are asphyxiated, or take the other advice, "Don't keep animals."

I tried mopping the floors with a rather strong solution of creolin but it did little good. Previous experience with pyrethrum was not very satisfactory. Knowing the volatility of naphthaline in warm weather and the irritating character of its vapor led me to try it. I took one room at a time, scattered on the floor five pounds of flake naphthaline and closed it for twenty-four hours. On entering such a room the naphthaline vapor will instantly bring tears to the eyes and cause coughing and irritation of the air passages. I mention this to show how it acts on the fleas. It proved to be a perfect and effectual remedy and very inexpensive, as the naphthaline could be swept up and transferred to other rooms. So far as I am concerned the flea question is solved and if I have further trouble I know the remedy. I intend to keep the dog and the cats.

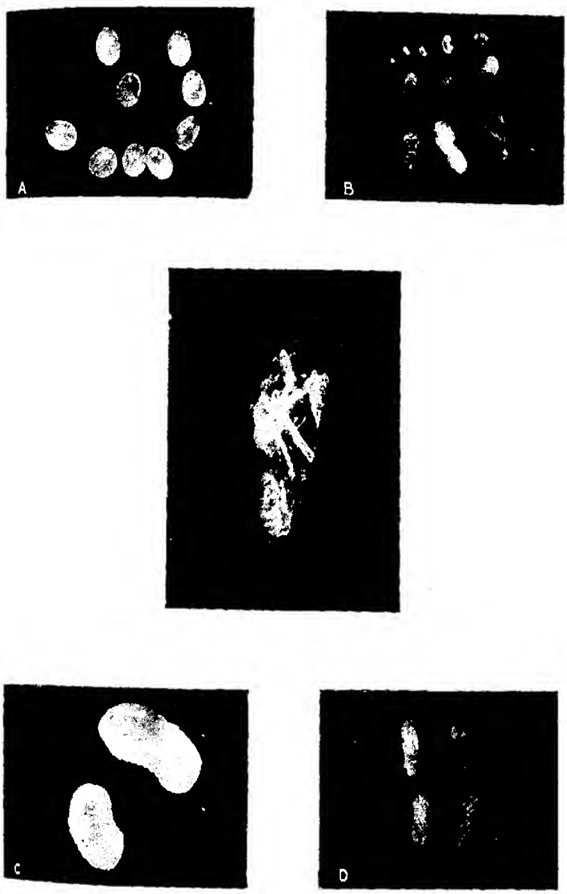
Henry Skinner, M. D.

Kerosene Emulsion for Terrapin Scale. A number of soft maples, literally loaded with *Eulecanium nigrofasciatum* Perg., were sprayed April 25, 1908, with a lime-sulfur wash, using 20 lbs. of lime and 15 lbs. of sulfur. The spray was applied hot and very thoroughly, about 10 gallons being used for each tree having a trunk diameter of 8 to 10 inches. Similar trees were then sprayed with a stock solution of kerosene emulsion, made seven months before and diluted to make a 20% solution. Many of the twigs with a diameter of over ½ an inch were completely covered with scale. The temperature at the time of application was about 60° F. and the leaves were nearly the size of a fifty cent piece. About 45 minutes after the oil application there was a terrific rain storm, lasting 15 to 20 minutes. The kerosene emulsion killed very few leaves, while the lime-sulfur wash did no injury.

A small colony of Argentine ants in their formicary (x 1.4). The queen is seen near the center of the picture and a short distance to her left can be seen a worker carrying a small mass of eggs (from photo by T. C. Barber).



"Formicarium," or special insectary, constructed and equipped by the author for studying the Argentine ant. Cages and formicaries, resting in large trays of running water, are seen to the right and at the further end is seen the combined hygrograph and thermograph, which makes a continuous record of the humidity and temperature in the room.



Immature stages of the Argentine ant: A, eggs deposited by queen, enlarged 20 times; B, worker larvæ and pupæ in various stages of development; C, full grown worker larvæ, enlarged 12 times; D, worker pupæ, enlarged 5 times; central photo is that of a male pupa, enlarged 11 times.

Limbs taken from the sprayed trees and checks showed that the lime-sulfur wash killed no scales, while the kerosene emulsion destroyed practically all.

CHAS. R. NEELLIE, *Cleveland, Ohio.*

Membracid Eggs in an Apple. In November, 1908, from Des Moines, Iowa, I received an apple which had in its skin a number of Membracid egg pouches similar to those made by *Ceresa taurina* Fitch. These pouches were just beneath the skin of the apple and nineteen were counted when the apple was received. Several had already been removed by the sender, Mr. W. H. Kinkennon, who first noticed them. A row of thirteen extended in a nearly straight line from the "equator" of the apple to a point near the calyx, with six more in a line part way around the calyx end. All of the pouches were placed with the long axis parallel to that of the apple. The egg pouch itself was an oblong swelling in the skin of the apple, having a small slit lengthwise at the lower left-hand side (the apple with the stem end upwards). There was no discoloration of the apple skin in the vicinity of the egg pouches.

The following measurements, made with a Leitz compound microscope (objective 2, ocular 3), were taken from one of the egg pouches: The pouch was 2.54 mm. in length; from .43 to .54 mm. wide, the narrow measurement taken at the end with the slit; the slit itself, 1.08 mm. long. Away from the slit the pouch widens slightly, as the measurements show, and make the slit somewhat pear-shaped. The egg itself is pale, almost translucent, and appears broader at the end farthest from the slit. The measurements taken of the egg are as follows: Length, 1.81 mm.; width, .35 to .47 mm.

The apple containing the eggs was sent to Professor Herbert Osborn, who confirmed the opinion of the writer, that the eggs were those of some Membracid, and possibly those of *Ceresa taurina* Fitch.

R. L. WEBSTER, *Ames, Iowa.*

Anthrenus verbasci Linn., a common museum pest, feeds, as is well known, upon a considerable variety of dry animal and vegetable substances. April 4, 1902, two ears of corn infested by this insect were received and placed in a 2-quart Mason jar and kept tightly closed, with no moisture aside from that in the somewhat dried corn. Breeding has continued apparently uninterruptedly during a period of seven years. The bottom of the jar is nearly covered with fine, white globose particles, apparently starch grains falling from the eaten kernels of corn and a thick mass of the brown larval skins and other debris. It appears from the above that this insect is capable of breeding for an extended series of years under such adverse conditions.

E. P. FELT.

The editors will thankfully receive news items and other matter likely to be of interest to subscribers. Papers will be published, so far as possible, in the order of reception. All extended contributions, at least, should be in the hands of the editor the first of the month preceding publication. Reprints of contributions may be obtained at cost. Minor line figures will be reproduced without charge, but the engraving of larger illustrations must be borne by contributors or the electrotypes supplied. The receipt of all papers will be acknowledged.—Eds.

JOURNAL OF ECONOMIC ENTOMOLOGY

OFFICIAL ORGAN AMERICAN ASSOCIATION OF ECONOMIC ENTOMOLOGISTS

APRIL, 1909

It is with a sense of deep personal loss and with the most sincere regret that we chronicle in this issue the decease of another leading economic entomologist. The Grim Reaper has in the past year laid a heavy tax upon our associates. Our loss in practical entomologists during this period has been equal to if not greater than that for the preceding decade. This process is bound to continue and can be partially stayed only by the recognition of our physical limitations. May we all be equally fortunate as our recent associate in winning a high place in the ranks of the profession.

There are dangers, grave dangers, in exactitude. This is particularly true where precise statements are made in a very emphatic manner. Some years ago a well known entomologist wrote: "Never use the gas stronger than 0.25 gramme cyanide per cubic foot on any kind of nursery stock." A recent bulletin changes this recommendation with no note or indication of emendation to .0088 oz. of potassium cyanide per cubic foot. This precise and somewhat remarkable recommendation appears in a bulletin ostensibly designed for practical farmers. There is no doubt that many agriculturists could figure out the proportion. There has been in the last few years an effort made to simplify our formulæ and to adapt them, wherever possible, to the requirements of practical men. The original recommendation of 0.25 gramme per cubic foot is not particularly appalling, though in a country where the avoirdupois system is in general use it occasions more or less trouble. The originator of this recommendation, we feel, would be disturbed if not startled, to find himself practically quoted as advising .0088 of an ounce. This instance appears to be a case where a man has singularly failed in adapting an otherwise sensible recommendation. It would have been plainer to have written 1 oz. to about 114 cubic feet of space, if it was consid-

ered necessary to maintain this scrupulous degree of accuracy. Furthermore, the general recommendation of most entomologists of 1 oz. to 100 cubic feet of space, a proportion abundantly justified by experience and one appealing strongly to the practical nurseryman and farmer, appears to have been entirely overlooked. Exactitude is commendable, scientific accuracy is desirable, but neither are advanced by the use of large decimal figures, unless unavoidable, in popular bulletins. We should never forget that the general adoption of recommendations by the entomologist depends in large measure upon their appealing to the practical sense of the parties charged with their execution.

Obituary

MARK VERNON SLINGERLAND

Mark Vernon Slingerland, Assistant Professor of Economic Entomology in Cornell University, died of Bright's disease at his home in Ithaca, March 10. His health had been failing for some time, but to most of his friends his death was unexpected.

Professor Slingerland was born in Otto, Cattaraugus County, N. Y., on October 3, 1864. He was a son of Jacob A. and Mary (Ballard) Slingerland. He was educated in the Otto village school and in the Chamberlain Institute at Randolph, N. Y. In 1887 he entered Cornell and in 1892 he was graduated with the degree of Bachelor of Science in Agriculture. He obtained special mention for special study with marked proficiency in entomology during the last two years of his course. From 1890 till 1904 he was assistant entomologist in the Agricultural Experiment Station, and in 1899 he was appointed assistant professor of economic entomology.

Professor Slingerland was a member of the Holland Society of New York, the American Association of Economic Entomologists (of which he was president in 1903), the Entomological Association of Washington, the National Mosquito Extermination Society and the Society of Sigma Xi (vice-president of the Cornell chapter in 1903 and 1904), and a fellow of the American Association for the Advancement of Science.

Professor Slingerland married, in 1891, Miss Effie B. Earll, who was a special student in the university in 1889–91. She survives him, with one daughter.

Although Professor Slingerland had barely reached middle life,

he was recognized as being one of the foremost workers in economic entomology, and had attained an international reputation.

He was a prolific writer. He had published many bulletins, and had contributed much to the periodical press, especially to *The Rural New Yorker, Country Life in America, The American Agriculturist, The National Nurseryman, Entomological News* and *The Canadian Entomologist*. He was a contributor to the Encyclopædia of American Horticulture and to the Encyclopædia of American Agriculture. At the time of his death he had in preparation a volume entitled "Insects Injurious to Fruit," which was to appear in Macmillan's Rural Science Series.

The position he attained was reached by untiring industry and a devotion to truth. His work was characterized by painstaking thoroughness and an absence of anything sensational. His constant aim was to determine the exact and complete truth and to present what he discovered in a clear manner. In this he was very successful, both in the class room and as a writer.

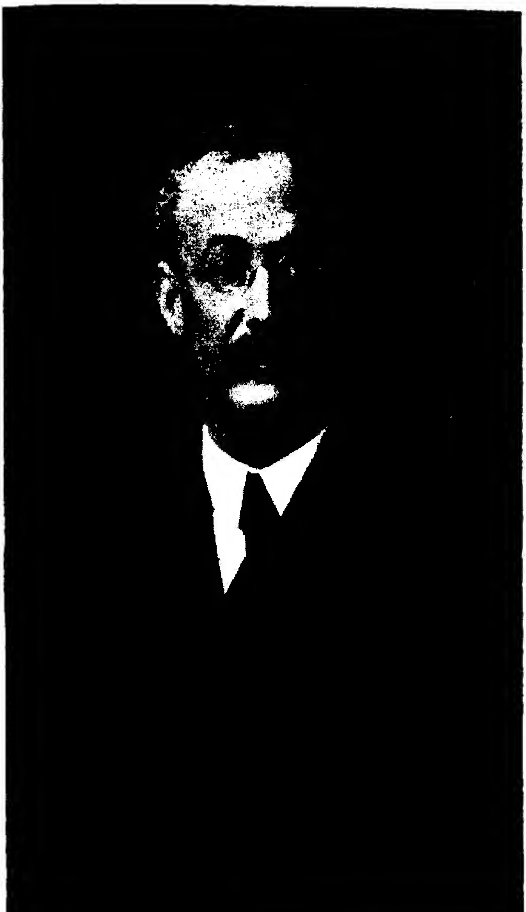
The bulletins that he published were in a marked degree monographic. Instead of writing about many insects, he selected a few and discussed them thoroughly, working up so far as possible every detail in the life-history of the species studied. It was doubtless this feature that caused his work to be so widely known in other lands. For example, his treatise on the Codling Moth was translated into Russian and published in that language.

Professor Slingerland took an active part in various scientific and horticultural societies. He was president of the Association of Economic Entomologists in 1903; chairman of the entomological section of the Association of American Agricultural Colleges and Experiment Stations in 1903; chairman of the committee on entomology of the Western New York Horticultural Society 1895–1904 inclusive; and chairman of the committee on entomology of the New York State Fruit Growers' Association in 1903.

As a teacher he was clear, direct and painstaking. He had the keenest interest in the needs of each individual student. Only a few hours before his death he discussed with a colleague the work of several of his students. Even at that hour, when it was evident to others that the end was near, his thought was not of himself but of his students.

In this manner closed the life of one who, although given but few years to work, accomplished much, and who endeared himself to others by his sterling qualities as a man and a friend.

J. H. COMSTOCK.



M. V. Slingerland

Reviews

Report of the Entomologist, by LAWRENCE BRUNER, Neb. Sta. Brd. of Agric. Rep't., 1908, p. 287–341.

This report gives summarized accounts of a large number of injurious forms, the chinch bug, the army worm, the rose chaffer, the clover-hay worm receiving the most attention. Professor Bruner's assistant, H. S. Smith, gives a summarized account of the spring grain aphis, *Toxoptera graminum*, and discusses briefly a number of other injurious species. Mr. Myron H. Schwenk contributes a paper on the bot-flies affecting live stock in Nebraska, the ox bot-fly, *Hypoderma lineata* being discussed in detail.

Eighth Report of the State Entomologist, 1908, by W. E. BRITTON, Conn. Agric. Expt. Sta. Biennial Rep't., Prt. XI, p. 763–848.

This is another of an excellent series of reports dealing with the economic entomology of Southern New England. It is stated that the conditions are more favorable than ever for the extermination of the gypsy moth in that state. A serious outbreak by the spring and fall canker worms justifies the extended account of these insects accompanied by biological observations. A key is given for the separation of the more injurious species affecting cucurbitaceous plants, each being discussed in a summary manner. The elm leaf beetle is given a detailed notice on account of serious injuries. The value of the report is greatly increased by a large series of admirable original illustrations.

Spraying Apples, by H. A. GOSSARD. Ohio Agric. Expt. Sta. Bull. 191, p. 102–25, 1908.

This gives in detail a series of experiments designed principally to determine whether or not the drenching sprays, so much emphasized in the western states, can be profitably employed in the eastern sections of this country. The author concludes that very heavy applications of poisoned bordeaux, within a week or ten days after the blossoms fall, will do much toward producing a high percentage of sound fruit. It should be observed, however, that these results were obtained by drilled-out Vermorel nozzles and not by the Bordeaux nozzle advocated by Professor Melander. Professor Gossard finds that by omitting bordeaux from the first treatment after blossoming and using only arsenate of lead, the danger of "russeting" apples is much reduced. The practical orchardist will be highly gratified to learn that as a result of Professor Gossard's experimental spraying, a profit of $1,400 was realized.

The Boll Weevil Problem, etc., by W. D. HUNTER, U. S. Dep't. Agric. Farmers' Bull. 344, p. 1–46, 1909.

This bulletin summarizes in comparatively few pages the outcome of extended investigations conducted by the Bureau of Entomology. Under remedial measures, the author emphasizes first the destruction of the weevils in the fall by uprooting and burning plants and also destroying all trash in the cotton fields and in adjacent localities where the weevils are likely to

hibernate. Fields should be located where damage can be avoided so far as possible. The above measure should be supplemented by wide and early planting in well prepared ground. A new agricultural implement devised by Dr. W. E. Hinds and known as the chain cultivator is figured and described. This implement is designed particularly to pulverize the surface soil and at the same time to work the infested squares toward the middle of the row, where they would be more exposed to the sun and the weevils more likely to perish.

What Constitutes a Perfect Stand of Cotton When Fighting the Boll Weevil, by WILMON NEWELL, La. St. Brd. of Agric. Immigra. Spec. Boll Weevil Bull. No. 1, p. 1–15, 1909.

The author calls attention to the fact that a perfect stand of cotton with boll weevil present is very different from what constituted a perfect stand before its advent, since this pest does not permit the plant to "make" during an entire season. A summary of the experiments shows that on upland prairie and bottom land during two very different seasons the average yield of closely planted cotton was 46% or 282 lbs. more per acre than in widely planted fields. This bulletin is particularly interesting, since it gives concrete statements respecting the effects following modification of agricultural treatment in an effort to control an insect pest. It is well within the province of the entomologist to determine the practical outcome of his recommendations.

The Saddled Prominent, by EDITH M. PATCH, Me. Agric. Expt. Sta. Bull. 161, p. 312–50, 1908.

This is a detailed account, based on the literature and original observations and with a bibliography, of *Heterocampa guttivitta*, a species which has been very destructive in Maine to both forest and fruit trees. The author in discussing the control of this species emphasizes the necessity of depending upon natural enemies, such as birds and other animals. The value of the bulletin is greatly enhanced by a series of original illustrations.

The author in bulletin 162 (p. 351–68) gives biological notes on a number of injurious species. Among the more interesting is a new spruce Tortrix, *Argyroploce abietana* Fern, a new Noctuid for apple, *Crocigrapha normani*, the European *Deilephila gallii*, *Eriophyes fraxiniphila* Hodgk. and *E. fraxini* Nal. on ash. The bulletin is illustrated by a number of original process plates. The latter would have been much improved had a better quality of paper been employed.

The Mosquitos of the Philippine Islands, by CLARA SOUTHMAYD LUDLOW. A Thesis Submitted to the Faculty of the Graduate Studies of the George Washington University, etc., p. 1–65, 1908.

This thesis consists of a large number of records showing the connection between the prevalence of mosquitos and the incidence of malaria. *Culex fatigans* is stated to be a host of *Filaria bancroftii* and more than suspected as a host of *Filaria philippensis*, while Ashburn and Craig claim it as a host for Dengue. *Mansonia uniformis* is given as a proven host for *Filaria bancroftii* in Africa. Four Anophelinæ, *Myzomyia funesta*, *M. ludlowii*, *Myzorhynchus barbirostris* and *M. fuliginosus*, are listed as probable malarial hosts in the Philippines. An extended bibliography accompanies the paper. The

data the author has brought together will prove of great service in determining the economic relations of a number of species.

The Tussock Moth in Orchards, by W. J. SCHOENE, N. Y. Agric. Expt. Sta. Bull. 312, p. 39–49, 1909.

This bulletin discusses an outbreak by the white marked tussock moth in orchards near Lockport, N. Y. An unusual though not unprecedented feature was the eating of the young fruit by the caterpillars. The author gives preference to the collection of the egg masses and spraying with arsenical poisons. The value of this bulletin is greatly enhanced by an excellent series of original illustrations.

The Peach Tree Bark Beetle, by H. F. WILSON, U. S. Dep't. Agri., Bur. Ent. Bull. 68, Prt. 9, p. 91–108, 1909.

This is an extended discussion based upon original investigations of *Phlœotribus liminaris* Harris. As control measures, the author tentatively advises severe trimming of all badly infested trees, burning the infested branches, and the application of a thick coat of whitewash three times each season to those slightly infested. A fine series of original illustrations adds greatly to the value of the bulletin.

San Jose Scale in Oklahoma, by JOHN F. NICHOLSON, Okla. Agric. Expt. Sta. Bull 79, p. 67–88, 1908.

This is a general discussion of the San José scale, accompanied by experimental data on methods of controlling the same. We seriously question the wisdom of advising, in a popular bulletin, .0088 of an ounce of cyanide potassium per cubic foot of space, in the face of the fact that the employment of one ounce to 100 cubic feet of space has given such universal satisfaction.

Home-Made Soluble Oils for Use Against the San Jose Scale, by J. L. PHILLIPS, Va. Agric. Expt. Sta. Bull. 179, p. 77–88, 1908.

This bulletin gives formulæ for the preparation of several soluble oils, together with the results of a series of experiments. The author advises the experimental use of these substances against San José scale and maple scale, and concludes that these mixtures can be prepared at home for about ⅓ the cost of the commercial article.

Current Notes

Conducted by the Associate Editor

Dr. Fr. Schwangart, who was recently appointed chief of the zoölogical section of the state experiment station for the study of fruit and viticulture in Rhenish Bavaria, is making an investigation of the insects injurious to these crops. He desires to secure American publications on such insects and would greatly appreciate any such publications that are sent to him. Address, Dr. Fr. Schwangart, Neustadt a. d. Haardt, 9 Maximillianstrasse, Rhenish Bavaria.

Mr. F. W. Urich has been appointed entomologist to the board of agriculture, Trinidad, British West Indies. Address, care of Board of Agriculture.

Mr. C. C. Gowdey, who was graduated from the Massachusetts Agricultural College in 1908, has been appointed government entomologist to Uganda Protectorate in Northern Nigeria, British East Africa.

Mr. F. W. Lowe has resigned from the Bureau of Entomology to accept a position with the Parke Davis Company. Detroit, Michigan.

Mr. C. B. Hardenberg, who has spent several months in Denmark, has returned to this country and is carrying on investigations on cranberry insects in Wisconsin for the Bureau of Entomology.

Mr. D. L. VanDine, who was entomologist to the Hawaiian Agricultural Experiment Station, has been transferred to the Bureau of Entomology. He will work on insects affecting sugar cane and southern field crops. Address, Dallas, Texas.

Prof. Trevor Kincaid of the University of Washington, Seattle, Washington, will sail for Europe in April. During the summer he will make extensive collections of the parasites of the gypsy moth in Russia and the material will be shipped to the Gypsy Moth Laboratory at Melrose Highlands, Mass. The investigation is being conducted under the direction of Dr. L. O. Howard.

The Ottawa Field Naturalist Club is receiving subscriptions for a permanent memorial which it proposes to erect in honor of the late Dr. James Fletcher. Suggestions as to the form of the memorial are as follows: *a*—A fountain at the Central Experimental Farm; *b*—a statue to be placed in the grounds of the new Natural History Museum; *c*—a bust or portrait to be placed in that building or at the Central Experimental Farm; *d*—To found a bursary at some Canadian university. No decision can be reached until the approximate amount of money available is known. Subscriptions may be sent to E. R. Cameron, Chairman, or W. Hague Harrington, Secretary, Ottawa, Canada.

Mailed April 15, 1909.